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E. H. B.

TRANSACTIONS

OF THE

EPIDEMIOLOGICAL SOCIETY

OF

LONDON.

NEW SERIES. VOL. I.

SESSION 1881-82.

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AIDS TO EPIDEMIOLOGICAL KNOWLEDGE.

By GEORGE BUCHANAN, M.D., F.R.S., President.

Inaugural Address of Session 1881-82.

(*November 2nd*, 1881.)

Gentlemen,—Our Society enters to-day on the thirty-first year of its life. It has experienced its ordinary share of difficulties, and, I venture to say, its full measure of success. In no small degree its difficulties have been the creation of its success. At its foundation it could be said that "scarlatina, typhus fever, small-pox, measles, etc., which are said to have destroyed 12,000 annually in London alone," were at the moment "but little thought of." That is happily no longer true; and the greater concern that is felt about those diseases, the sense of their importance to the community, of the power of the community to limit or extinguish them, and of the duty of the community to exercise all means having this object that it can use; all these are results to which our Society has contributed in a foremost degree. We have now other Societies, public bodies and their officers, governments general and local, competing with us in their interest for the problems which we give ourselves to study. But we continue, I venture to say, to hold our place as pioneers, ever trying to push forwards the frontiers of knowledge, while welcoming every fellow-settler in the countries that we make it our business to explore.

We have among us, and I trust we always shall have among us, people who study the problems of Epidemiology from very different standpoints. Out of doubt we are primarily a society of medical practitioners, who are wishful, for the sake of their science and its beneficent applications, to exchange experience concerning that great class of diseases which afflicts people, not as individuals only, but as groups and multitudes, and which is, beyond all other classes, the one that we have learned to regard as capable of limitation and prevention. As medical practitioners, we follow at the bed-

side and in the *post-mortem* room, in the hospital and the prison, in the camp and in the ship, the natural history of these diseases; we learn what we can of them, and we bring our lesson here. Or we look to records of disease, in England and towns of England, in Europe and in countries of Europe, in India, in America; we examine the time, the place, the circumstances, of what is "epidemic"; and we bring, for this Society's instruction, our conclusions as to the relation of all things, animate and inanimate, with the production or absence, the propagation or extinction of epidemics and epidemic diseases. But we want at every moment, for the successful pursuit of our object, to study the δημος under other aspects than the technically medical, to study other δημοι than our own, and to identify ourselves with those who so study them, with statesmen, with geographers, with travellers; we want, too, help from mathematics, from chemistry and physics, from meteorology, botany, zoology: indeed it would be hard to name a department of human knowledge to which we do not hope some day to become indebted. And thus, the medical science that is at the foundation of our Society, is the science of medicine in its most comprehensive sense, and there has ever been a place in our constitution for workers outside the medical profession, willing to work towards a better understanding of epidemics.

I propose, in meeting you at the commencement of our new session, to speak of one or two ways in which we as epidemiologists are called upon to make applications of this wider knowledge, and in which we more especially value the aid that is given by the special student to the actual practitioner.

The first idea that must occur to anyone who hears me is the place which, among all such subjects, we shall gratefully assign to mathematical science. If in physiology and therapeutics, we have recourse to this science for the composition of our facts, for a knowledge of the general from the particular, for the apprehension of degrees of certainty under conditions that are inconstant and not wholly known; the indebtedness of epidemiology to mathematics begins at an earlier and ends at a later period of its course. So identified is the one with the other science, that there has occasionally been a confusion in the popular mind between the two things, and the study of epidemics has been regarded as essentially an affair of statistics: while, on the other hand, it has sometimes happened that laborious and promising work in the field of epidemiology has been without result, owing to want of apprehension of some circumstance that is plain enough when it is once pointed out by the mathematician.

From the nature of the case, disease which can be named from its *epidemic* quality, must claim in a peculiar degree the aid of numbers for its investigation; but it would be a serious error to think of this relation as ending with a record of quantity of disease, of death rate, and sickness rate in various places at various times. Arithmetical science can help much further than this with epidemics. It is very largely by its aid that lessons can be learned as to the cause of disease in communities. It is by arithmetical analysis of coincident circumstances, of one and another sort, that a clue is often got for the epidemiologist about the relation of effect to cause, that answer may be had as to sufficiency of cause to produce effect, and that probability is measured of cause being discoverable or not in some half understood group of conditions. The case of relation between cholera and elevation of soil, is an example that is now familiar to us, of an experience noted by the arithmetician, to have something in it for the physician to enquire further about, and of the physician finding in his further study of it what it really was that lay at the bottom of the observed relation. Of another sort of arithmetical help, a charming instance was once afforded to myself during the investigation of a fever outbreak; when of two competing sets of circumstances, either of which, it was thought, might contain the cause of the outbreak, I was told that the probabilities in favour of the one set were as three hundred and seventy-five to one, while the probabilities in favour of the other set (putting aside any probabilities against it) were but as six to one. In the case then in question, I had good data respecting attacks of fever and not only about deaths: and I shall have your concurrence in thinking that it is by the mathematical physician, by him who most clearly sees what arithmetical science can do for etiology, that the notion of getting a registration of sickness as a larger basis for his study than can possibly be afforded by any mere registration of death-causes, will be regarded as of peculiar importance.

Another illustration of aid to epidemiology from mathematics. An investigation is being made as to the possible influence of an infectious centre upon its neighbourhood. Supposing there to be such a centre, nobody supposes it would be equally operative at all distances. Putting aside countervailing influences that may act in destroying the infection, there is necessarily more of it at a spot near the centre than there is at a spot farther off; that is, so long as the distribution of the infection is effected after the fashion of a something radiating from centre to circumference. But

at once there arises the question: *How much* more infection will there be at a given nearer point, than at a given more distant point?—because an answer to this question is wanted before we can go on to consider the meaning of the facts we may observe, before we can see how any observed distribution tallies with our hypothesis of radiation from centre to circumference, and before we can begin to judge of the presence or absence of countervailing influences. In reply to this question, we shall at once see that the distribution of our infection will be effected in a different fashion, according as it is along a single plane or into space of three dimensions. In the former case the relative amounts of infection present at two given places will be inversely as the distance of each place from the centre; in the latter case, they will be inversely as the squares of the distances. Now we have no reason for assuming the distribution to be only along one plane; but there is reason for thinking that the contagious matter is subject to the laws of gravitation, and that it is not susceptible of diffusion after the manner of a gas. Wherefore it is to the plane of the earth's surface that the contagion will tend, and its distribution in that plane will come, by the operation of gravitation, to be effected according to the law that governs radiation in one plane; the quantity on a spot of given size becoming less as the distance of the spot from the centre increases. There is no longer any question of the diminution being in the ratio of square of distance. In this way we can appreciate the rate at which the spread of infection should manifest itself, provided one and another condition were fulfilled; and we are then in a position to go further, and inquire as to their fulfilment or non-fulfilment, and to understand the significance of the actually observed distribution. Examination of the influence of infected rags over the inmates of a sorting room, or of a hospital upon the houses of its neighbourhood, is obviously best undertaken after clear notions have been formed on elementary points of this kind.

I feel that I owe our Society an apology for taking over-simple illustrations of my contention that we must always work hand-in-hand with the mathematician. In such, and in more difficult cases, we are our own mathematicians, and not a few papers in our Transactions shew us to be so. But precisely for this reason, and because instances are plentiful elsewhere, in which lack of attention to such rudimentary matters as these has led to error in epidemiology, I have felt bold to use this occasion for shewing, by instances which he who runs may read, the dependence of our subject upon mathematical aid.

The best example I can take of our indebtedness to *physical* science may be chosen, not from among the multifarious cases where varying facilities for the passage of matters through soils have determined the occurrence of an epidemic, or where the fashion of flow through pipes has led to the distribution of infective matters among one part of a community while another part has received none of them, but from the lessons that the physicist has taught us about the very characters of infection itself. At the foundation of our Society there was but a half-formed recognition of the fact that every disease of the class with which we concern ourselves has a material of its own, and still less recognition was there of the qualities of the material of disease. Medicine had accepted the *deleterium quid* of Morton as comprising what need be known of the material of epidemic disease, and thirty years ago had hardly got beyond the notion of the "airs" and "humours" of the ancients as sufficing for its production. Enough if a name could be given to the attribute; the character, qualities, of the thing were barely thought of. The earlier addresses of our first President contain many exhortations to the use of the microscope as a means of understanding the nature of the diseases which we had to study; and from the microscope in the hands of Dr. Beale we soon came to know of the existence of particulate matter in infective liquids. But it was not until the resources of the physicist had been applied to separate out the particle from the fluid, and until the one and the other had been handed separately to the experimenter, that any conclusion could be formed about the inherence of the quality of contagiousness in the one or the other element of the infectious liquid. The method of diffusion was used by M. Chauveau to such liquids, and first to vaccine lymph; everything soluble was separated from what was particulate; the two portions were used separately in the operation of vaccination, and the potency of the lymph was found to be in the particulate, not in the fluid, portion. What was more, it was found that not only contagia which are always contained in liquids, like vaccine or farcy, but also contagia that, like small-pox or sheep-pox, show their effects through the air, exhibit this same quality of being separable into two elements, a soluble and diffusible, and an insoluble and indiffusible part, the one of which possesses while the other does not possess, the property of infecting a susceptible body. From this fact as to the very substance of contagion existing as insoluble particulate matter, or in necessary connection therewith, other conclusions appear to follow. It had long been observed by the practising physician that the severity of a case of disease

resulting from an exposure to infection was not a mere affair of the distance from source of infection, or of the length of time of exposure. And from the consideration that contagious matter must be conveyed as particles are conveyed, this common observation received a large measure of explanation. In experimenting with the contagious liquid of sheep-pox, dilution to 10,000 parts did not alter the nature of disease resulting from inoculation. Only the chances of any disease at all being produced by the inoculation were reduced from practical certainty to a moderate uncertainty. We cannot understand how this should be the experience of a *soluble* contagion, where the effect must needs be dependent on the amount of dilution which the contagious matter had undergone.

At the time that this demonstration by the physicist of the particulate nature of contagia came to assist the inquirer into the nature of epidemics, the pathologist had been cautiously tracking other lines of research. He knew, as regards the inoculable members of his group at all events, that he was dealing with some organic material which, if taken from one diseased body and put into another, has the power to excite in that second body the same morbid phenomena as those of the first, and he was inquiring whether it might not be through some proper vital endowments of its own that his organic material possessed this property. So long ago as 1841, a happy inspiration had led Dr. Farr to insist upon the use of the word "zymotic" as best expressing the character common to epidemic, endemic, and contagious diseases; but at that time ferments had not, any more than contagia, been discovered to have a vital action concerned in their chemical doings. And then, in reasoning upon the cholera epidemic of 1848-9, Dr. John Snow had satisfied himself that the best explanation of the phenomena of cholera and its spread was to be found in the existence of a specific cholera "cell", capable, after its discharge in evacuations, of passing, by the vehicle of water or air, into other bodies, and producing cholera in them. The aggregate of facts on which Dr. Snow founded his argument were seen to be insufficient for complete proof, and difficulties in the way of his contention were indeed perceived by himself; but he was not daunted by the objections of those who would not follow him into the domain of natural history, wherein, he insisted, the analogies of the matter of cholera were to be sought. So, further, Dr. Budd was cautiously working towards the same conclusions, and had reached the judgment, alike for diseases propagated through air and for

those communicated by means of water, that "the contagious agent which issues in the specific excreta is the fruit of its own prior reproduction [he takes his analogies from vegetable life] within the already infected body."

There could be no better illustration of my contention that epidemiology requires the co-operation of every other science, than the fact that while this was the state of knowledge respecting the intimate nature of contagion, the great step in its advance was made by the chemist. In study of the essential nature of various fermentative and putrefactive processes, it has appeared to Professor Schrœder in Germany, and M. Pasteur in France, that certain microscopic bodies were essential to such changes; bodies that, discovered in one such process, were distinguishable in their characters from the bodies discovered in another such process; bodies that pass through certain changes of their own in the course of the chemical changes of fermentation or decay, the changes of their own being of a kind that implied vital qualities, and stamped the microscopical objects with the attributes of animal or vegetable structures. From the standpoint thus reached by the chemist, the chemist himself came to regard similar organisms as probably concerned in processes of disease also, and the researches of M. Pasteur in 1865 into the characters of the disease which was then so fatal to silkworms in France, gave sufficient demonstration that this was a true view of the case in respect of that disease, and afforded a stronger presumption than had ever before been justified, that the material of contagion was, in nature, a parasitic being, having a life and life-history of its own.

This strong presumption, taken with the doctrine of the particulate nature of contagion, rapidly led to further inquiries. One after another the characteristic staff-shaped bodies of splenic fever and the spirillum of relapsing fever, as well as the microzymes of vaccine, of small-pox and of sheep-pox, had been made out, and observers were debating as to the precise share taken by these particles in the production and dissemination of each several disease; the more cautious pathologist hesitating to accept the conclusions of the more advanced, who would rank them, not as mere carriers, but as actual generators of infection. In our own country the papers of Drs. Sanderson and Klein of 1870-1874, while adding to the sum of knowledge on the subject, were of the highest value also in warning against over-hasty generalisations respecting it.

For years before this period, the suggestion that particu-

late contagium must, almost of necessity, be contagium vivum, had been one that had, as was to be expected, its attractions for another helper in epidemiology—for the botanist. What if the living particle concerned in disease should be, not a mere microscopical organism, having no other form of life than as a cellule or a filament; but if it should be in fact the reproductive matter of some vegetable thing, capable of growth and development under other circumstances into another form, in which it may lead another life? What if, in its other form, it should have plant-characters of its own and be capable of pervading, after the fashion of known parasites, some animal or vegetable tissues; and what if animal or vegetable substances thus pervaded should possibly enter into the food of man? At one moment, in respect of the material of cholera, it did seem that we should find the truth about the genesis of that disease to lie in this direction. It looked as if the microzymes of cholera could be cultivated into other minute vegetable forms, and they in their turn to cysts, which were indistinguishable from the fructification of a urocystis that is parasitic on the rice plant. Given varying conditions of temperature and food to determine simple mycelium-growth on the one hand, or fructification on the other hand, and it seemed to be a mere question of opportunity whether the cycle of the plant's life should include the production of formidable disease in the human subject. It has to be admitted that Dr. Sanderson's critical examination of Professor Hallier's experiments relegated to the domain of the unproved this promising vision into the nature and cause of cholera; but as time has gone on, we are coming more and more to expect that the materials of some contagia have another habitat, perhaps in a known but not yet recognised alternative form; and the pathologist is always looking, with eyes of welcome and criticism, to the help of the botanist.

As time has gone on, indeed, and as one disease after another has been found to be accompanied by organisms that are apparently concerned in their pathological processes; as not only this and that skin disease, but the several members of the contagious and epidemic group have been brought with more or less of probability into the list of those which have parasitic vegetable organisms for their actual cause, we are invited further to observe, in the natural history of the various organisms, affinities of botanical class broadly correspondent to the nature of the groups of diseases with which they are found associated. We are told of external parasitic diseases being related to minute plant-forms

which are grouped together by recognisable points of structure of their own, so as to make for the botanist a different group from those which are concerned with internal diseases of animals. Within the latter group, we hear of genera of disease each characterised by a particular genus of plant-form; of malarial disease, enteric fever, anthrax (infective diseases common in having more or less of relation to the *soil* of the place where they prevail), being characterised by bacillar forms of plant-life; while diseases of the exanthematic sort, septic processes, syphilis, glanders and others, exhibit as their speciality collections of micrococci in various grades of existence. Again, we are told of distinctions for the botanist between one and other species of these micrococci; of one sort requiring more and another requiring less oxygen for its development, and behaving in one or another way, according to this different want of oxygen during its artificial cultivation; the plants of each group, it is suggested, disporting themselves variously during the time that they inhabit the body which they have diseased, and so, by reason of the physiological requirements of each, determining what organ shall be affected, and the pathology of its condition, as mere circumstances in the natural history of the particular vegetable parasite.

Unquestionably, the prospects thus held out to the epidemiologist are of the most fascinating kind. As there was a period when the quality of communicability in disease seemed likely to come within the speculations of the chemist, and when we were promised, in the behaviour of metallic oxides in the presence of peroxide of hydrogen, the line of explanation of what constituted contagiousness, so now we seem to have reached a point where the botanist offers us the most promising insight into the nature of what is "epidemic". We are not indeed in a position to accept the whole of Professor Klebs's most interesting generalisations; but we do possess, in the researches of observers in Germany, in England, and in France, evidence that appears unmistakable of certain epidemic diseases having specific cultivable organisms for the actual materies morbi of each; and we are learning almost every day something new about the existence of like organisms in other communicable diseases.

I have thought that I could not render our Society a better service than by asking my friend Dr. Henderson, who succeeds to-night to the chair of our junior secretary, to prepare for our use a *résumé*, in some measure chronological, but in the main proceeding upon pathological classification, of

the progress that has up to the present time been made in investigations of the intimate nature of epidemic disease. Dr. Henderson has kindly complied with my request, and will favour us with a paper in this sense. It will have a value beyond its first professions, since it will contain a number of bibliographical references that will make it serviceable to those who desire to push forward their studies of this branch of our subject.

I wish, however, to spend the time that remains to me by pointing out some of the interesting and important consequences to us as epidemiologists, that follow from the doctrine which would regard the material of infectious disease as something in its essence having a place and mode of life beyond the infected body. One of these is the possibility that is appearing of our being able to cultivate that material outside the body, in ways that shall give it different degrees of potency when introduced into the body. So long ago as 1867-68, Dr. Sanderson, in experimenting on the relations of phlegmonous inflammation, had distinguished two sets of consequences producible by local inflammations artificially set up in the lower animals; on the one hand, a chronic disease having all the anatomical characters of tubercle; on the other hand, an acute disease presenting the leading features of pyæmia; both infective in the sense of inducing other similar inflammations in other parts, and the one or the other being producible at the will of the experimenter by the inoculation of the same material. In 1872, he demonstrated, in a number of acute infective processes, microzymes abounding in the exudation liquids and in the blood; microzymes of various form, yet probably all organically continuous; of one form in the more acute and intense, of another form in the more chronic infective liquid; and in 1875 he had shown how, by successive transmissions of infective material, an intensely virulent material could be obtained from an inflammatory product that had begun with none but ordinary qualities, the original product and the cultivated material having certain microscopical and physical characters distinguishing the one from the other. In 1875, Dr. Koch of Wolstein, made a very notable step in advance. He found that the bacilli of splenic fever would grow and multiply, not only in the tissues of the living animal, but in blood serum or aqueous humour, and even in artificially prepared cultivating liquids; that this could be done through several generations, and that at the end, inoculation of an animal with a minute quantity of the cultivating liquid produced the same disease, the original splenic fever,

in a susceptible animal. More than this—he found that mice inoculated with matter that contained numerous spores died of splenic fever in twenty-four hours, whereas mice inoculated with liquid in which spores were scarce, did not die until after two or three days. And shortly afterwards, in 1876, our own Dr. Klein found this same property of cultivability outside the body, in the very similar bacilli of the disease of the pig, known as pneumo-enteritis. Liquid of the eighth generation inoculated into a healthy pig produced a disease in that pig identical in its *post mortem* characters with pneumo-enteritis; but the disease was so reduced in intensity that, during the fortnight the animal was allowed to live, it showed no sign of disturbance to its health. Three years later, at the end of 1879, Dr. Sanderson found that, in the case of anthrax, cultivation within the body of rodents, which do not succumb to the disease as bovine animals do, produced a change in the quality of the anthrax material. Direct inoculation from one bovine animal to another would pretty certainly result in the destruction of the successive animals; but inoculation of the material into the rodent and back again into the bovine creature, resulted in the production of a disease, serious it is true, but altered from its first intensity, inasmuch as it did not now kill. And it was found that the bovine animal, which had passed through this mitigated form of anthrax, was now protected against the occurrence of any but an unimportant local disease, if it was now inoculated direct with the material of spontaneous unmodified anthrax.

At the same time, M. Pasteur, advancing on the ground that had been won by Koch and Klein, made out for the disease that he calls "fowl cholera", the same quality of cultivability by multiplication of its bacilli in organic but not vitalised liquids; and he found, moreover, that inoculation of a healthy fowl with such cultivated material produced in it a form of *cholera des poules*, but a modified, not fatal attack; and this slighter attack was found to have the power of protecting against subsequent disease from inoculation of the virus of the original cholera.

From the time of these observations, as I have said, it would appear inevitable to regard the microphytes of such disease, and of any disease similarly comporting itself to the pathologist, as being the veritable *materies morbi* of the disease. And we cannot help in future having before us the dazzling prospect of being able to offer to men and animals the protection of a trivial disease against the risk of each more fatal infectious disease; to achieve what M.

Pasteur calls the "vaccination" of animals against the several dangerous contagia.

But perhaps as valuable, though hardly so obvious, are the indications we are gaining from such studies, of possible means of preventing epidemic disease in other ways. We see promise of understanding, as we have never understood before, how people may come by infections whose mode of attack has often puzzled us. Suggestions arise of the material of contagion having, like tapeworm, an alternative mode of life outside the animal body, or in one animal body leading a different sort of existence from its way of existence in the other animal body. What if the material of cholera, of yellow fever, of enteric fever, should have another habitat than as a contagium, and there exist as a commonplace organism, having, under ordinary conditions, no power of producing disease; but capable, when those conditions were changed, of altering its mode of life or its fashion of development, and becoming the active material of disease in animals that swallowed or inhaled it, or into whose bodies it was in some other way introduced? And what if we could recognise it in its other fashion of life and keep away from it, or destroy it, or prevent the occurrence of the conditions required for its development into a morbid poison? The hygienist would have a new prospect before him. Here is one of this kind of suggestion. In 1875, Dr. Klein was able to point out, in the particle which he had recognised as peculiar to enteric fever, microscopic appearances closely corresponding to a microphyte which had been identified and named by Professor Cohn of Breslau. This microphyte had been found by Cohn, not in any animal tissue, but in the water of a certain well in Breslau; and it is the fact that the district of the city supplied by that well had long been famous for the amount of enteric fever in it. With the later views of Professor Klebs before us, we obviously stand in need of further research, not about the Breslau well only, but in all instances where we can detect enteric fever in the course of being distributed from a particular water source. Again, the relation between milk and outbreaks of enteric fever, or scarlet fever, or diphtheria, has been commonly thought of as a mere affair of conveyance of disease-products derived from some antecedent case of disease, and entering milk-cans by the aid of water used in washing or of hands used in milking cows. But, knowing what we now know of the ability of specific particles to multiply in suitable media outside the body, we have strong reason for suspecting that the milk may have served as the cultivating

ground for some organism, and have acquired a potency altogether in excess of any power for harm that may have been given it by the mere entry of a stray particle of morbid matter; and more, it will be quite reasonable in the future to be on the watch for organisms developing in the milk into morbid poisons, but not having themselves been derived from an antecedent case of specific disease. It may turn out that disease-producing milk has received some organism that under other conditions of its life would have been impotent, but that when thus cultivated in milk may attain, perhaps with some change in its manner of growth, a new power for harm. Or such an organism may have been derived from the body of the cow herself or from her milk apparatus; and be in the cow incapable of producing any important general disease; yet be capable, when transferred to another kind of animal by way of the cow's milk, of producing serious specific disease in that animal.

Indeed the hygienist is coming to suspect that he may often have to do with organised material not far developed from perfectly harmless non-specific particles, but capable of cultivation in appropriate media into the matter of specific contagia. I have before mentioned Dr. Sanderson's cultivation of an ordinary inflammatory product into a virulent septicæmic poison; a few years later, in 1870, it appeared to Drs. Grohe and Block that the sporules of common penicillium injected into the blood of rabbits and dogs acquired new qualities that resulted in a fatal disease to the animal. So, later on, Nägeli affirms that common putrefactive fungi can under certain conditions develop into infection-particles, and they in their turn back again into the material of putrefaction. There is, indeed, considerable doubt about the correctness of these observations, and a recent very careful examination of them by Dr. Gaffky has led him to the belief that the microzyme of each disease is a living thing of its own proper species, arising only from its like and unable to produce anything but its own kind. And for the present it is undoubtedly safer to accept this position.

But it is impossible not to regard in this connection the inquiries of Dr. Ballard at Welbeck and Nottingham, in which he learned of strange maladies occurring in groups of people after the manner of acute specific disease, and having their cause for the whole group of people in some common article of food. A ham, or a leg of pork, dispensed to a score or several scores of people, has brought about the result of all or nearly all those people, after passing through a period resembling an incubation period, suffering from a smart diar-

rhœal fever; and when exact investigation of all the circumstances has been made, it has appeared (so far as data could be had) that the original meat, the people diseased by eating it, and animals inoculated from the diseased people, were infested alike by some minute bacillus that had common characters in every place where it was found, and that seemed out of question to be the actual element of the disease. The observation is assuredly of the greatest importance to the question of vital contagion, and suggests fresh lines of inquiry concerning the development of the non-specific to the specific.

Gentlemen, such are a few examples of the aid which pathology is rendering to an understanding of epidemics; it has seemed to me well on this occasion to record something of our progress on this side of our many-sided subject. And our progress has not been slow or inconsiderable. It must be of concern to our Society in the future, to have regard to the many new considerations that are presenting themselves to us, and to put the views of the pathologist to the test of our manifold experiences.

MALARIA.

By Sir JOSEPH FAYRER, K.C.S.I., M.D., F.R.S.

(*Read: Feb. 1st*, 1882.)

THE subject of the following communication was suggested by some allusions made by Dr. Ewart in his paper last month, and by remarks offered in reference to them, in the discussion that followed.

The question raised was the old and still unanswered one, What is Malaria?

It is not with any hope or intention of supplying the information we so much desire, that I address you, but, that after referring briefly to the present state of our knowledge, I would invite such discussion as may elicit your views and gather from your experience something that may throw light on this obscure subject. It is one that has often been discussed in speech and in writing, but I doubt if ever it has been presented to an assembly better qualified than this, by experience and knowledge of every kind, to do it justice, and to put before the world such facts and explanations as may clear away the doubts that now surround it. I know I am addressing men who have had the largest opportunity of witnessing the operations of malaria, who have studied the subject practically as well as by the light of rapidly advancing knowledge of micro-pathology, and who are capable, from every point of view, in which such a subject can be studied, of investigating and revealing its nature and characters. Therefore, I venture to place it before you for discussion as one, than which there can be none more appropriate for your consideration.

What do we mean by this term, which is so familiar everywhere, and here especially? one that means so much, and yet conveys so little. The Italian word is of comparatively recent origin, and is not the one in popular use in Italy either, for there they talk of the "Cattiva-aria", though that of course means precisely the same thing, and is the popular, as distinguished from the scientific, term.

It was in Rome, about two hundred years ago, that Lancisi first pointed out the connection of agues with marshes and their emanations, and called them fevers, and the exhalations

producing them, "paludal"—as they still are called. In Rome again, a successor of Lancisi traces the connection of ague with the marshes, but this time it is not to an exhalation, but an organism, which he and his co-worker Klebs call Bacillus malariæ. Which is nearer the truth, Lancisi or Crudelli, remains to be proved; but of this more presently. Laveran has recently discovered another micro-organism in Algeria, which he thinks is the *vera causa.*

Malaria, whatever it may be, manifests itself, and is best known by its influence on man—and animals too, as I am informed by Profs. Axe and Fleming (though this is often disputed), over a great portion of the earth's surface. It has no defined geographical limits, but practically may be said not to pass the isothermal line 65° north, and 30° south latitude; and not to appear when the diurnal range of temperature is below 60° Fahr. It becomes more intense and active, generally, approaching and within the tropics.

It is very much regarded as a tropical evil, and so indeed it is, but not by any means exclusively so, for it is found all over Europe, in the south especially; and the Campagna of Rome, the Pontine marshes, the banks of the Danube, the fens of Holland, the rocks of Greece and Gibraltar, all maintain an evil celebrity.

In nearly all continental Europe there are malarious regions; Spain, Italy, France, Germany, Greece—even our own country is not exempt. Once we were a very suffering people, and lost two kings, a queen, cardinal, a lord protector, and many other great people, from a disease that is now almost unknown, at all events in its severe forms, though it still lingers in some districts of the south and east of England. Why this change? It comes of better drainage, better houses, better food, better personal and general hygiene, and therefore it is full of encouragement for other countries; and are we not now profiting by this experience, and thus gradually diminishing disease everywhere? Dr. Chevers, who for years has taught the Government and people of India how they might preserve their lives and health, will tell you that, by degrees, it is being recognised and shown that people need not die as fast as they did, even in India, and as we have reduced the mortality of soldiers, who are under control, from 60 to 16 per 1,000, from 13 to 3 of fever alone, so we may do, and indeed are doing, much more still; but how much remains to be done! About 3,500,000 natives die yearly in India of fevers. No doubt registration is still imperfect, though they are doing all that is possible to improve it, and deserve immense credit for what they have done,

and very probably 50 per cent. of these who die do not die of malarial fevers but of thoracic complaints, and others with a hot skin. Still, if one and a half million of people die of undoubted malarial fever, to say nothing of the other one and a half million, and of thousands who perish from dysentery, diarrhœa and other complaints, such as cholera according to some, etiologically linked with this class of malarial fevers, is it not prodigious? The great cause of this mortality is the unknown thing or condition or group of conditions, called malaria. Some call it climatic influences, sudden alternations of temperature, chill only. But is this the case? Can a chill, or can any agency irrespective of something ingested or inhaled produce such effects? Hippocrates and his predecessors probably knew that people who lived near marshes got intermittent fevers, but they did not know of malaria, or think of fevers and paludal exhalations, in connection with each other, as cause and effect. It was Lancisi who first attributed fevers to marsh exhalations. Agues, remittents, pernicious, masked fever, and other paludal cachexiæ have long been known, but they are not worse in the Tropics than they are now, or have been in Europe in past times. Read Pringle's valuable work, Lind, Ferguson, MacCulloch, and others of that time and at a subsequent date; or go back further, and read Lancisi and Sydenham, and you will see that not the coast of Guinea or the Nepaul Terai can give you anything much worse than Europe did in those times, and could do now under certain conditions we hope never to be revived. Read Pringle's account of the state of the army in Flanders, or even in Scotland before and after Culloden, and you will understand that tropical fevers differ more in intensity than in kind from fevers in temperate climates. Clarke says, that in Calcutta in 1780, about 80,000 natives and 1,500 Europeans died of fever annually. In 1880, 3,797 died, all told, of fevers, chiefly malarial.*

* *Prevalence of Fever and Causes.*—The registered deaths from all causes in India in the year 1879 were 4,975,042.

Cholera accounting for	270,552
Small-pox accounting for	194,708
Bowel complaints accounting for	250,173
Fevers accounting for (out of a population of 187,105,833)	3,564,035

Civil Population, Fever Deaths.

1877	2,504,493
1878	3,247,371
1879	3,564,035

or thirteen times as much as cholera; though it may probably fairly be

What then is the nature and *raison d'être* of this mysterious agent, so destructive to our race and yet so amenable to sanitary interference, and which all over the world, under certain atmospheric and telluric conditions, produces so much

estimated that not more than 50 per cent. of these deaths are due to endemic fevers.[1] In the case of certain classes subject to registration, the figures are reliable; those relating to the general population are probably less trustworthy, but still sufficiently accurate to give a tolerably correct idea of the prevalence of disease and extent of mortality.

Let us look at the statistics of fever-prevalence as illustrated by the sanitary reports and returns of hospitals in Calcutta.

The mortality from "fevers" in Calcutta has been, during six years:

1875	...	...	5,328	1878	...	...	6.186
1876	...	...	4,361	1879	...	...	4,796
1877	...	...	5,151	1880	...	...	3,797

Clarke says that in 1770, 80,000 natives and 1,500 Europeans died from fever in the city of Calcutta.

The British army in India in 1879 numbered 57,810 men; of these 51,959 suffered from fever, with a mortality of 387.[2] The Native army,

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[1] In the present state of registration it is not possible to define the special character and type of these fevers. They are certainly, for the most part, malarial in character. Attempts have often been made to arrive at the actual number of true fever-deaths, and they all agree in showing that fatal diseases attended by heat of skin and other febrile characters are returned as fevers. In the Chanda district, out of 1,008 deaths, the Civil Surgeon found that 672, or 66.6 per 100, were due to fevers properly so called. The Civil Surgeon of Betul reports that out of 208 deaths 66, or 31 per cent., were fever deaths, and so on. The Civil Surgeon of Mandla states that from personal inspection he has ascertained that fever forms 7.5 per cent. of the mortality. The Civil Surgeon of Wardah gave the following result of personal inquiry into the cause of 69 reported fever-deaths.

18 died of remittent fever.	2 died of pleurisy.
9 died of bronchitis.	2 died of peritonitis.
8 died of ague.	3 died of vermes.
3 died of continued fever.	

Suppose 50 per cent. die of fever, the loss of life is great.

[2] The mortality of the British troops in the Bengal Army during sixteen years, 1830 to 1845, gave a ratio of 13.25 per 1,000 from all fevers.

The same for 1875 was only 2.77 per 1,000.
,, 1876 ,, 2.46 ,,
,, 1877 ,, 2.21 ,,

In 1878-79 there was more fever from the famine and Afghan War. These three years appear to be normal.

Army of all India for five years, 1871-75:—

Deaths from all fevers, 1875=2.81 per 1,000.
,, ,, 1876=2.41 ,,
,, ,, 1877=2.16 ,,
,, ,, 1878=5.07 ,,
,, ,, 1879=6.55 ,,

The rates for 1878-79 were part of the great fever epidemic which swept over India and occasioned enormous losses to the civil population. The rates include the Afghan fever and those on the march of troops.

disease and mortality that it has been called the destroying angel, whose mission it is to keep the population of the world in check; producing disease too of a peculiar type, and having the power of establishing a diathetic condition in

of 130,011 men, had 122,375 cases of fever with 756 deaths. The jail population, of 117,680 persons, had 73,484 cases of fever with 1,306 deaths. 1879 was an unusually unhealthy year, epidemic fevers of a malarial type were prevalent and fatal. In some districts, during and after the close of the rains and beginning of the cold season, the mortality was very high. In Bolundshur and Allyghur the deaths rose to about 113 per 1,000 of population, the mortality from all causes being but little in excess of that of fever alone [1]

The population of Bengal, under registration, in 1880 was 59,890,237; about 4 per cent. located in towns, 96 per cent. in country and villages. The deaths registered in the whole province were:—

Cholera	39,643
Small-pox	22,953
Fevers	689,605
Bowel complaints	44,969
Injuries	22,339
All other causes	103,124
Total registered deaths	922,633

Fevers destroyed nearly three times as many as all other death-causes put together.

BOMBAY.

Seasons.	Registered Fever Deaths.	
	1880.	Mean of 14 years.
Cold	93,800	63,446
Hot and dry	38,488	28,304
Rainy	73,746	62,992
Hot and damp	40,745	38,773
Total	246,779	193,515

The Deaths for each month being

January	24,689	July	18,922
February	22,625	August	19,856
March	23,921	September	18,396
April	19,901	October	18,406
May	18,579	November	22,339
June	16,572	December	22,565

The seasonal prevalence (*vide* Table) shows how cold and variable

[1] "The unusual rise in the fever mortality was marked in August and September, was at its height in October and November, and then gradually declined" (*Sanitary Commissioners' Report* for 1879, page 78). The Sanitary Commissioner of the North-West Provinces remarks:—"Everywhere the rise, culmination, and decline happen at the same time, and as from a cause co-existent everywhere in the Province. This cause was malaria, and it is attributed to excessive rainfall following two years of comparative drought" (*Sanitary Commissioners' Report* in 1879, page 78).

those who have been exposed to its influences that shall affect them for the rest of life, and reproduce on the banks of the Thames that which was inaugurated half a century before on the shores of the Hoogly, the Indus, or the Congo?

Marsh miasm and marsh malaria are inappropriate names, however, for a disease-cause that is often generated where there are no marshes, and which is often as active in its manifestations on sandy, rocky, arid ground as in swamps. It has been shown over and over again, that the desert plains of north-west India, as well as arid regions in other countries, where there is no apparent surface water, and little or no vegetation—such as on the granite rock of Mount Aboo, on the magnesian lime-stone of Kurachee, the Island of Ascension, Rock of Gibraltar, and other similar localities—these fevers of a periodic type abound, and that a soldier may come away as fever-stricken from Peshawar as from the Soonderbunds. No part of the 1,600,000 square miles of India seems to be quite exempt, except perhaps the higher hill stations, and even they are doubtful; it is thought by some that above 3,000 to 5,000 feet it is absent, but this is a mere approximation. It is the fashion to speak of malarial and non-malarial localities, but in India, in the sense of producing fever, all are malarial. If there be one thing of which, by evidence, we are more convinced than another, it is that somehow water-logged land, *i.e.* land where the subsoil water is near the surface and stagnant, is the chief determining cause. The surface may be dry as it was at Walcheren, but if it be porous, and the air can get at the stagnant water, with perhaps ever so little organic matter below, and if there be a temperature above 60° Fahrenheit, then the state of things, or the thing itself we call malaria, may occur. This seems to be proved by what occurs in the Doab, between the Ganges and Jumna, and in other districts where irrigation canals bring in more water than is needed for the wants of agriculture; the land becomes water-logged, and in the absence of drainage, the fevers pre-

temperatures affect the etiology of these fevers. The contrasted mortality between that of the hot and dry, and the rainy and damp seasons shows the effects of added moisture on the amount of fatality. The fevers in these returns are all placed under one general heading—the types comprehended being the malarial and continued. Among them, no doubt, are included deaths due to other inflammatory disorders, such as pneumonia; whilst, on the other hand, not a few of the deaths from dysentery and diarrhœa belong more properly to malarial fevers. Registration is rapidly improving, but is not yet perfect.

vail in a degree unsurpassed by any part of the country, except perhaps in the Terai.*

For example: The population of the North-west Provinces and Oude in 1881 was 44,107,061; the deaths from fever amounted to 987,220, nearly a million. The death-rate in the Terai was nearly 53.41; in Roy Bareilly 21.72. The irrigated districts of the Doab, which come next, are nearly as bad as the Terai, and we have had, for some years, opportunity of watching the effects of subsoil water in excess in producing fever in the province of Burdwan, in Bengal, which became simply water-logged by arresting

* *The Sanitary Commissioners' Report* of December 31, 1880, contains much information in respect of the prevalence of fever, the causes of malaria, and the influence of irrigation on fevers. This province includes the pestilential Terai and the Doab, an irrigated area which was scarcely less unhealthy.

Population of Oude and North-West Provinces, 44,107,061 in census of 1881. The registered deaths for five years, including the period of scarcity, was:—

Years.	Total Deaths.	Deaths per 1,000 population.
1876	937,490	21.94
1877	840,538	19.67
1878	1,521,724	35.65
1879	1,914,499	44.81
1880	1,281,155	29.99

The improvement in 1880 follows relief from scarcity. The lowest death-rate in 1880 was 21.72 in Rai Bareilly. The highest in the Terai 53.41. The chief death-causes in 1880:—

Diseases.	Total Deaths.	Deaths per 1,000.
Cholera	71,546	1.67
Small-pox	8,240	0.19
Fevers	987,220	23.11
Bowel complaints	80,312	1.88
Injuries	20,553	0.48
All other causes	113,284	2.65

About twenty-three out of every twenty-nine deaths were due to fever. The mortality caused by small-pox, cholera, and dysentery, is nothing in comparison with that from fever. Nearly a million of people (987,220) died in 1880 of malarial diseases. The liability to fever here seems to have been increased by other predisposing causes, the most important being under-feeding; but as this was not the case with the entire population, and as they were not all badly clothed and housed, though all suffered, it is evident that there were other causes in operation.

The Sanitary Commissioners' Report for 1879 shows that the general causes which influenced the public health in fever localities, were undrained ground into which canal-water had been led, and rainfall added to the already water-logged subsoil. In 1880, food became cheaper, and except in certain districts there was less rain. In 1879, during the great scarcity of food, the fever death-rate was 37.82 per 1,000; in 1880, it fell

or altering the natural course of streams and interfering with subsoil drainage. It was nearly depopulated by a low form of fever, call it malarial, typho-malarial, or what you will. Certainly the greatest weight of evidence seems to support the theory of stagnant subsoil water, and organic matter at a certain temperature, as the cause. But the subsoil water must be STAGNANT to make it dangerous. When it moves, as it often does, ever so little, it is no longer so detrimental. Some would exclude the necessity for organic matter at all, considering moisture and earth to be sufficient without it; this may be so. Others think, or suggest, that animal, combined with vegetable organic matter, may have peculiar effects, according to the relative quantity of each. What tropical marsh exhales vegetable decomposition alone? It

with increase of food to 23.11 per 1,000, which was still above the five years' average of 20.91 per 1,000.

The monthly fever mortality for the whole registration area was:—

North-West Provinces, 1880.

Months.	Fever Deaths.	Months.	Fever Deaths.
January	116,366	July	56,502
February	72,030	August	74,127
March	69,250	September ...	87,618
April	72,534	October ...	91,248
May	76,022	November	99,459
June	78,200	December	93,264
Total	485,002	Total	502,218

The total annual deaths from fever, taken through the scarcity period were:—

1877	574,722	1879	1,616,108
1878	982,117	1880	987,220

The fundamental cause of the great loss of life from fever, was increased predisposition from scarcity of food. Cold, damp, and alterations of temperature in the latter half of the year, and dampness of soil, the result of irrigation throwing more water into the subsoil than was needed by growing crops. This must have acted as a serious predisposing cause.

The chief engineer was of opinion that the fever is *not* due to irrigation, but to great diurnal range of temperature, chills, and imperfect feeding. He says:—"Without the great diurnal range, canal irrigation will not produce malarial fever;" but, he admits, that under these climatic influences it may do so, especially in the winter months, and thinks that better clothing and food would protect the people. But we know that these fevers occur irrespective of changes of temperature, though, no doubt, they have much influence in re-exciting it in those who have previously suffered, but not *de novo*. Fever occurs in every month of the year, though more in some seasons than others. With the excessive mortality in irrigation districts, the conclusion is inevitable that the true cause is stagnant subsoil water. The remedy for all this is better drainage, whilst no more water should be used than is required for the crops.

must be mingled with the miasmata of decaying animal life of low forms.

If malaria be a miasm, an exhalation, is it one, or are there many? Does the same thing produce fatal remittent fever and slight malaise? Is one, only the result of concentration, the other of dilution, of the poison? Is the malaria of a granite rock, or of a sandy arid plain like Sinde, or parts of Spain, the same as that of a marsh, or a mangrove swamp? How does it differ from that of a rubbish-filled tank, dry ditch, moat, drain, cesspool, pond, tank, some upturned earth, mud, or soil, exposed to the light and air after a jungle has been cut down, the bilge of a ship, etc., etc.? MacCulloch says malaria comes from every pond, ditch, hole, coppice, wet grass, or ground in England, and that the malaise is experienced by those who live in damp houses in the neighbourhood of ponds, or which are surrounded by moats, and so on; or even a flower-pot, a watered flower-bed, is only different in degree from that which strikes a man down in an algid pernicious fever, or coats his tongue and teeth with sordes like that of typhus.

All these are questions unanswered, but such as must suggest themselves to those who have had to deal with the matter practically, as well as to those who only deal with it statistically. No one has seen or found any tangible or particulate thing as yet that can be relied on. I come to Klebs, Crudelli, and Laveran presently, "La terre est riante et fertile, une fraîcheur délicieuse vous repose le soir des chaleurs brûlantes du jour—et tout cela c'est la mort." You remember the description of the "cattiva aria", in the Villa Borghese, by Madame de Stael. Chemists have done their best; many foreign matters have been detected, gases, organic matter, and so on, but none of these is the malaria. The Italians have lent us two words—malaria and influenza—both expressive of much, yet descriptive of nothing. Influenza is more vague even than malaria, for it deals with a more complete abstraction. If an impression made through the cerebro-spinal and vaso-motor system from without by heat, cold, electricity, or from within by an emotion, or in any other way by which the body may be affected, can produce acute catarrhal, bronchial, intestinal, febrile, or nervous symptoms, why may not such a cause explain the phenomena ascribed now to malaria, as an entity, *i.e.*, a gas or a germ?

Some such view is held by highly experienced men at present, and was in past times by others. Read what Ferguson said last century; what Drs. Gordon, A. Smith, Moore, Oldham, and

others say now. There certainly is much in the character of Indian fevers to support these views, which are eminently deserving of consideration. For my own part, I must say that I cannot feel convinced that merely climatic influences, alternations of temperature, abstraction of heat, can account for a tertian or remittent fever. There must be, I think, a "tertium quid", but I do not know what it is. Chills, cold, and damp will reproduce ague in one who has suffered before, or will develop it in those who have lived in a so-called malarious country, though they never had fever when there; but it will neither cause fever *de novo*, nor that peculiar cachexia with which many of us are so familiar.* Now, whether this subtle addition is a chemical molecule, or an organic germ; an emanation, or a bacillus; or whether it is only the outcome of the forces which surround us, and of certain external influences acting on the cerebro-spinal centres, and producing certain nutritive changes, and perhaps developing an autogenetic poison, is the question which needs solution.

The interesting researches of Klebs, Crudelli, Laveran, and their Italian and French followers, as Dr. Henderson told us not long ago, in a very able summary of the most recent researches into this branch of science, are the latest attempts to define malaria. According to Klebs and Crudelli, bacillus† seems to be at the root of this as of so many other evils; if so, what a monster he is despite his littleness! Think of a microscopic rod causing half the mortality in the world! People thought badly enough of the cobra for slaying 20,000; the bacillus slays his millions yearly. But does it really do all this? Is it really the *teterrima causa?* Is all the periodic fever, dysentery, some would say cholera, diarrhœa, cachexia, spleen, beri-beri, etc., due to this microphite? Dr. Sternberg, who has repeated Klebs and Crudelli's experiments with great care, says that he does not deny the possible connection of fever with bacillus, but the causal relation is not yet proved. Still the weight of evidence at present seems to be in favour of the bacillus; but think of all it will have to explain.

Those who have lived in India or other malarial countries and dealt with malarial diseases, and have seen the extraordinary caprices and results ascribed to malaria, will find it hard to explain on this basis. For my own part, I am not

* The great tendency now-a-days is to trace all disease to a specific cause, but we must not lose sight of the possibility of causes developed in the body, and altered conditions of the nerve-centres and vaso-motor action, and so on, by forces influencing from without.

† According to Laveran, a ciliated corpuscle.

convinced. But I am convinced that we are infinitely indebted to those who are working out these difficult and interesting problems in such a truly scientific spirit, and that it would be most presumptuous to assert that their researches will not be successful in solving this venerable puzzle. What, then, best meets the case? What will best explain all the seeming contradictions? Will a gas, an aeriform imponderable thing, or a germ imperceptible to our microscopes or in our test-tubes, or will the result of a variety, or combination of abnormal conditions which are capable of acting on the centres and vaso-motor mechanism, so as to induce internal congestion, structural visceral change, or those nervous phenomena, alterations of temperature and nutrition, that are comprehended in an attack of fever, do so? This is the question that we want to solve.

Meanwhile, I content myself, provisionally, with the working hypothesis of an entity "malaria", and speak of *it*, and its influences, as though it really were a particulate thing. Klebs and Crudelli's bacillus theory is beautiful, and explains as much, if not more than, any theory that has preceded it; still it wants confirmation. Theories of an organic cause are not new; from Vario and Lucretius down to Linnæus, thence to Salusbury and others, such views have been put forward and again been laid aside, but never stated with such force or probability as by Klebs and Crudelli; and these are already disputed. I do not know yet what Indian research has done in this matter, but it is in competent hands. As it stands, we must accept the bacillus theory with caution; for however much we may be inclined to do so, it would be as rash to accept it as established, as it would be presumptuous to assert that it will not be so.

IS THE CLIMATE OF INDIAN HILL SANITARIA BENEFICIAL IN SCROFULA, TUBERCULOSIS, AND PHTHISIS?

By Deputy Surgeon-General JOSEPH EWART, M.D.

(*Read: January 4th*, 1882.)

In a short paper communicated to the Society last session, an endeavour was made by me to demonstrate the comparative exemption of the natives of the Indian peninsula from scrofula, tuberculosis, and phthisis. This freedom was attributed to the nature of the food consumed, open-air life, warm temperature, the infrequency of measles, mumps, whooping-cough, scarlatina, etc., and last, though not least, to the remarkable absence of the scrofulous diathesis. But, notwithstanding these circumstances, it was found, after close scrutiny of Bryden's Tables, that the British soldiers and their families enjoyed no such immunity; that, in fact, phthisis was as prevalent among them, whilst located in India, as in their comrades and their families at home, or the soldiers of the United States of America. This unexpected result was assigned to inherited scrofula, liable to be developed into activity by repeated attacks of malarial fever and its manifold consequences, and the prolonged operation of extreme heat, eventuating in a lowered tone of the vaso-motor nerves, indigestion, anæmia, mal-assimilation, and imperfect nutrition. The conclusion arrived at was that the plains of India did not afford an advantageous residence or field of occupation for scrofulous exotics, or for those suffering from incipient phthisis or tuberculosis.

It now remains to extend this investigation to the hill stations as permanent or temporary resorts for the scrofulous or phthisical. Physically, India may, as Dr. Hunter suggests, be divided into three great regions. "The first includes the lofty Himalaya mountains, which shut it out from the rest of Asia, and which, although for the most part beyond the British frontier, form an overruling factor in the physical geography of Northern India. The second region stretches southwards from the base of the

Himalayas, and comprises the plains of the great rivers which issue from them. The third region slopes upwards again from the edge of the river plains, and consists of a high three-sided table land, supported by the Eastern and Western Ghauts which run down the coast on either side, till they meet at a point near Cape Comorin. The interior three-sided table land, thus enclosed, is dotted with peaks and ranges, broken by river valleys and interspersed by broad level uplands. It comprises the southern half of the peninsula."

For the purposes of this inquiry, India may be divided into those regions which are malarious and hot, and those which are non-malarious and cool. The first embraces all the plains and low-lying lands of the three great Himalayan river systems of the Indus, Ganges, and Brahmaputra; and of the subsidiary rivers to the east, west, and south of Vindhya and Satpoora mountains; or from the sea-level to an elevation of three or four thousand feet. The second division includes mountains ranging from 3,000 or 4,000 to 7,000 or 8,000 feet in height or more. In order that a just conception of the influence of the sanitaria comprised in this division may be formed, it is necessary to furnish information regarding climate, altitude, barometric or air-pressure, thermometric range, humidity and rainfall, compiled from the *Report of the Meteorology of India*, in 1878, by Henry F. Blandford, F.R.S.; *The Imperial Gazetteer of India*, by W. W. Hunter, LL.D., C.I.E.; and *The Health Resorts of India*, by W. J. Moore, Deputy-Surgeon General, C.I.E., Bombay Army, and supplemented by my own personal experience.

1. *Abbotabad.*—The average height of this civil and military sanitarium is about 4,100 feet. It is situated in the Western Himalayas, surrounded by pine-clad mountains 9,000 feet high, and is distant about 40 miles from Murree. It is hot in summer and cold in winter. The rainfall is 46 inches. Vicissitudes of temperature are great and sudden during the hot and winter months. From December to February snow and frost prevail, with a cutting northerly wind. "Owing to sudden changes of temperature, the station is trying to weakly persons, especially if delicate about the chest, or if subject to any form of rheumatism or neuralgia" (Moore).

2. *Murree.*—This station is 7,307 feet above the level of the sea. The mean annual barometric pressure is 23.850 deg. The highest monthly average is 23.958 deg. in November; the lowest 23.737 deg. in July. The mean daily temperature for the year is 55.7 deg. January is the coldest month, when the mean temperature is 36.7 deg., whilst June is the

hottest, with an average of 71.0 deg. The mean daily range for the year is 14.3 deg., the minimum being 11.1 deg., in February, the maximum being 17.5 deg., in October. But in March, April, May, June, September, November, and December, it stands at 15.0, 14.6, 14.1, 16.7, 15.5, 15.4, and 15.3 degs. The absolute range for the year is as much as 72.7 deg. In August it was 23.6 deg., and in June 43.9 deg. In winter, frost and snow are frequent. The upper part of the station is exposed to northerly winds. The lower division is sheltered from all winds excepting those blowing from the west. Measles, whooping-cough, croup, pneumonia, and bronchitis prevail among children. On the whole, the climate is well suited for Europeans who have not contracted organic disease.

3. *Simla.*—This is the summer headquarters of the Government of India, lat. N. 31 deg. 6 min., long. E. 77 deg. 12 min. Average elevation, 6,952.74 feet. The meteorological record for this sanitarium is incomplete. The average barometric pressure for the year is 23.41 deg., varying from 23.138 deg. in July, to 23.363 deg. in November. The mean annual temperature is 54.5 deg., the range wandering from 10.8 deg. in August, to 20.2 deg. in November and 24.1 deg. in April. The absolute range for the year is not recorded, but it is probably not far short of that given for Murree. In April it was 47.7 deg. The rainfall was 70.20 inches. In May, June, July, August, and September, the fall amounted to 4.56, 7.90, 19.36, 18.17, and 6.27 inches. During this period the climate is prejudicial to all suffering from pulmonary diseases, dysentery, and liver affection. Hill diarrhœa is also common, especially among delicate children and adults enfeebled by exposure to the great heat and malarious poisoning incidental to previous and prolonged residence on the plains. To most persons free from organic disease of the lungs, liver, colon, and heart, etc., the hot weather and winter are bracing and exhilarating.

4. *Kussowlee.*—This is a military sanitarium, about 32 miles from Simla, and 6,322 feet above the level of the sea. Under ordinary circumstances it is healthy; but it is exposed to epidemic outbreaks, cholera having occurred in 1845, 1857, 1867, and 1872. It is warmer than Simla. During the rains it is exceedingly damp and foggy. In the winter months it is cold and changeable, with occasional frosts and falls of snow. It is considered to be highly beneficial in functional derangements consequent on malarial fevers and exposure to the enervating influence of extreme heat. Cases of organic affections of the liver, intestines, heart and lungs are not

favourably influenced by residence at this station. Sanawar, a plot of ground in the Simla district, "made over by the British government, in 1852, as the site of the Lawrence military asylum", is situated on a hill facing Kussowlee, from which it is three miles distant. The children enjoy remarkably good health, and there is reason to believe that it is prophylactic against the active expressions of tuberculosis and phthisis to a material degree.

5. *Subathoo.*—This is also a military sanitarium, nine miles from Kussowlee and 23 miles from Simla. It is 4,500 feet above the sea level and is very hot in summer. Cases of phthisis do not do well here during the rainy season.

6. *Chakrata.*—This mountain station, founded in May 1866, and occupied by European troops in April 1869, is situated in the Dehra Dun, North-Western Provinces, on a range of hills overlooking the valleys of the Jumna and the Tons in the region known as the Jamsa'r Ba'war. It is 7,051 feet above the level of the sea. The mean annual barometric pressure is 23.227 deg.; the lowest monthly mean being 23.121 deg. in July, the highest being 23.313 deg. in November. The mean annual temperature is 55.7 deg., the lowest monthly mean being 39.6 deg., in January, the highest being 70.2 deg., in June. The mean annual range is 14.3 deg. During the rainy months of July, August, and September, it is 11.4, 9.6, and 11.3 deg. In February, March, April, May, June, October, November, and December the mean monthly range is 13.8, 17.4, 16.1, 14.9, 16.1, 14.3, 15.3, 17.0 deg. The absolute range for the year is 59.7 deg. The annual rainfall is 59.96 inches, about the same as at Murree. Rain falls in every month. The register shows that the greater part falls in June, July, August, and September, viz., 7.03, 17.36, 15.8, and 5.89 inches.

7. *Mussoorie.*—This station, situated on a mountain facing the Dehra Dun, is, according to Blanford, 7,433 feet above the sea level. But the greater part of the inhabited portion of the sanitarium varies in elevation from 6,500 to 7,000 feet. There is no record of the barometric readings before me; but seeing that the elevation is almost the same as Simla, or about 75 feet lower, the air pressure may be taken as being proportionately less than 23.41 deg. The mean daily temperature is only given for the eleven months ending with November. The lowest mean monthly temperature is 42.1 deg. in January, the highest in June, 74.2 deg. The lowest monthly range is 8.5 deg. in August, the highest 16.3 deg. in November. The absolute range varied from 18.0 deg. in August, to 38.3 deg. in April. The neighbouring sanitarium

of Lândour, being 700 feet higher, with a northerly exposure, is about 4 deg. cooler than Mussoorie. The annual rainfall at Mussoorie is 95.21 inches. In the rainy months of June, July, August, and September, it amounted to 9.43, 29.68, 30.75 and 9.47 inches. Rain falls in all the months of the year. Persons suffering from the heat and malaria of the plains, and disturbance of digestion consequent thereon, are cured by residence at Mussoorie and Lândour. But organic affections of the heart, lungs, liver, and bowels are aggravated. There is much less liability to hill diarrhœa than in some of the other Himalayan sanitaria. It has been stated that phthisical persons are favourably influenced during the hot months by the southern aspects of Mussoorie. This opinion has probably been based on the behaviour of such cases in different aspects and elevations of the station itself. But that this result has ever been regarded as so favourable in this class of disease, as to warrant much deviation from the general practice of abstaining from recommending patients suffering from phthisis to be sent to the sanitarium, does not appear in the evidence at my disposal.

8. *Ranikhet.*—This military sanitarium is situated in the Kumaon District, North-Western Provinces. It affords more level land for building purposes than any of the other Himalayan sanitaria. Its elevation above the sea is 6,068 feet. The barometric pressure is 24.058 deg.; mean annual temperature, 60.3 deg.; mean daily range, 13.5 deg.; absolute range, 36.6 deg.: and rainfall, 47.63. It is admirably suited for all those functional derangements resulting from the acute and chronic phases of malarious poisoning. But it is not deemed to be of much real service in organic affections of the heart, lungs, liver, and colon.

9. *Naini Tal.*—This station is also in the Kumaon District, North-Western Provinces, and is the summer head-quarters of the local government. "It is picturesquely situated on the banks of a beautiful little lake, which nestles among the spurs of the Himalayas" (W. W. Hunter). Its elevation is 6,409 feet. The mean annual temperature is 58 deg. In summer, the heat seldom rises higher than 80 deg. In winter, frost and snow prevail. The mean variation is from 78 to 48 degs. in May, 69 to 59 degs. in June, 78 to 61 degs. in July, 76 to 62 degs. in August. The rainfall for the year is put down at 81.10 inches, but it has been as much as 140 inches. In the monsoon months, the climate is, therefore, relaxing, and in the winter, snowstorms and cold raw winds are frequent. Hill diarrhœa is less common than at Simla. Rheumatism, neuralgia, croup, bronchitis, and

pneumonia prevail, particularly during the cold months. It is, like most other hill stations of corresponding elevation, unsuited for liver, bowel, heart, and phthisical complaints.

10. *Almora.*—This station is situated in the Kumaon District, 5,494 feet above the sea. Owing to want of shade, and the usual postponement of the rains, the heat of summer is considerable. The greatest monthly temperature is 88 deg. in May; the lowest, 36 deg. in January. The annual rainfall is 38.05 inches. In winter, frost and snow are common; but the snow seldom lies long on the ground. In this season neuralgia, rheumatism, and chest complaints are frequent.

11. *Darjeeling*—This sanitarium is situated on one of the crescentic ridges springing from the Singhalelah range in Sikkim. It is now in direct railway communication with Calcutta, and constitutes the summer residence of the Government of Bengal. Its elevation is 6,912 feet above the sea. The barometric pressure for the year is 23.373 deg. The lowest air pressure is 23.276 deg. in July, and the highest 23.473 in November. The mean annual temperature is 54.6 deg.; the lowest monthly average being 39.2 deg. in January, the highest being 63.3 deg. in June. The mean annual range is 13.8 deg. It is 10.6 deg. in July and August, 10.4 in September, 17.2, 15.7, 14.2, 15.8, 15.3, and 16.8 degs. in March, April, May, October, November, and December respectively. The absolute range for the year is 53 deg. The minimum absolute range occurs in the month of July, when it stands at 17 deg.; the maximum in March, when it amounts to 35.9 deg. The annual rainfall is 118.24 inches. It has been known to amount to 150 inches. It is, perhaps, the most salubrious of all the Himalayan sanitaria. Much of this is doubtless due to the comparative equability of its climate and a fairly good water-supply. It also enjoys considerable immunity from wind and storms. Its general salubrity is demonstrated by its beneficial effects on children. Weakly children transplanted thither from the plains, even when they are not free from the taint of scrofula, soon become plump, firm and rosy. In most functional ailments contracted on the plains, both by the young and those of maturer years, residence at this station proves very advantageous; but for those labouring under organic disease of the abdominal, pulmonary, and circulatory organs, it is not beneficial. It enjoys considerable immunity from hill diarrhœa. Many persons find it useful in approaching the station to stay at the lower elevation (4,500 feet) of Kurseong for a few days, thus allowing the system to become somewhat gradually accommodated to the altered air-pressure and temperature.

12. *Cherrapoonjee.*—This station, now abandoned, on account of its extreme dampness and rainfall, except by the Welsh Calvinistic Mission, who still continue to do excellent work among the neighbouring tribes, is situated in the Khási Hills, Assam, and is 4,588 feet above the level of the sea. The rainfall for the three years ending 1876 averaged 368.41 inches. Blandford gives the average for twenty-two years at 492.45 inches. In 1861, it is said that it amounted to 805 inches or (67.1 feet), of which 366 inches were registered in the month of July! Cherra, as the station is popularly styled, stands "on the first of a series of hill ranges that rise abruptly from the plain of Bengal, and catch the vapour of all the clouds that roll up from the sea" (W. W. Hunter). Hence this extraordinary rainfall, exceeding, so far as existing knowledge goes, that of any other place in the known world.

13. *Shillong.*—This station is the headquarters of the Chief Commissioner of Assam, and also constitutes a military sanitarium for the troops serving in Assam and Cachar. It averages about 4,900 feet above the level of the sea. This height was selected because it was deemed high enough for native troops. But it is overtopped by a mountain 6,000 or 6,400 feet high, backed by a large quantity of gently undulating table land. This is quite unoccupied, as it was thought that it was too elevated to be grateful or beneficial to a native garrison. The highest monthly mean temperature is 81 deg. Frost is noted in January and December. Although only twenty miles from Cherra, the rainfall averages only 86.93. Rain falls in every month of the year. In April, May, June, July, August, September, and October, it amounted to 3.54, 10.24, 16.49, 15.92, 14.38, 15.58, and 6.21 inches. During the rains, the equability of the temperature is as marked as at Darjeeling. In the winter, however, the vicissitudes are greater. It is unsuited to bowel, liver, and chest affections.

14. *Pachmari.*—This station is situated on the Satpoora range, Central Provinces, and is 3,564 feet above the level of the sea. The air pressure is 26.413. The mean daily temperature for the year is 70.7 deg. The highest monthly mean is 83.1 deg. in June; the lowest 54.7 in January. The mean annual daily range is 18.0 deg.; the lowest monthly range is 7.4 deg. in August; the highest 26.4 deg. in December. In January, February, March, April, May, and November, the range is 20.7, 21.4, 25.8, 21.8, 21.0, and 21.3 degs. The absolute yearly range is 71.5 deg. In March it is 55.8 deg.; in December, 44.9 deg. Rain falls in every month of the year, the average for the year being 80.93

inches, of which 9.86, 30.99, 20.09, and 16.0 inches were measured in June, July, August, and September respectively. Owing to position and moderate altitude, the clouds and mists do not hang so heavily over this sanitarium, during the monsoon, as at most of those in the Himalayas. The range is considerable in the cold and hot months, and this alone condemns the station as a resort for those liable to chest affections. Although during the rains the range is moderate, the great dampness of the air is a serious drawback.

15. *Chikalda.*—This sanitarium, situated in the Melghat Taluk, Ellichpur District, Berar, is 3,656 feet above the sea. The air-pressure is 26.426 deg. in December. The mean annual temperature is 72.0 deg. The highest monthly temperature is 81.8 deg. in May; the lowest, 64.4 in January. The mean annual range is 14.6 deg.; the lowest being 7.3 deg. in August; the highest, 20.3 deg. in May. The absolute yearly range is 54 deg. The absolute monthly range is 13.4 deg. in August, and 44 deg. in March. The annual rainfall is 58.13 inches, of which 8.89, 15.13, 16.73, 11.17, and 3.73 inches were registered in June, July, August, September, and October.

16. *Mount Aboo.*—This station, for civil and military, is situated on a detached portion of the Aravalli range, which extends across India from Delhi to the confines of Guzerat, in the district of Serohi, Rajputana. Its average elevation is 3,945 feet. The barometric pressure varied from 25.817 deg. in July to 26.118 deg. in December. The mean daily temperature is 69.0 deg. for the year. The minimum monthly mean is 54.2 deg. in January; the maximum 77.9 deg. in May. The extreme summer heat is about 92 deg.; sometimes as much as 98 deg. The mean annual range is 15.4 deg., the minimum being 6.9 deg. in August; the maximum being 20.1 deg. in January. In February, March, April, May, June, October, November, and December, the mean monthly range is 16.4, 18.3, 17.2, 16.7, 16.1, 15, 18.1, and 19.1 deg. respectively. The absolute yearly range is 61.9 deg., the absolute monthly minimum being 16.2 deg. in August; the maximum being 43.9 deg. in January. Rain falls in every month of the year. The annual fall is 63.36 inches, of which 5.43, 20.63, 23.12, and 8.47 inches were registered in June, July, August, and September. Moore, in his *Health Resorts*, says it is malarious in the autumnal months, and attributes this to the soil filling depressions in the granite rock, of which the hill is mainly composed, and from which depressions there is no escape for the moisture, except by evaporation. But it is not improbable that the evidence furnished in favour of this

opinion, would, on further analysis, go to show that the malarious fevers there observed, were either primary or secondary attacks from poisoning, contracted by prior residence on the plains, and elicited by the greater range of temperature experienced in this, as in all other Indian hill stations, in the months immediately succeeding the rainy season. During the autumn and winter, bronchial attacks are common. During the remainder of the year, comprising the summer and rainy months, almost all maladies, excepting phthisis, liver, heart, and bowel complaints, are benefited.

17. *Mahableshwur.*—This, the principal sanitarium of the Bombay Presidency, "occupies the prolonged, and, in many places, almost level, summit of a range of the Western Ghats, from which it takes its name, with a general elevation of 4,500 to 4,700 feet". It constitutes the summer head-quarters of the Bombay Government. The mean annual temperature is 66 deg., with a daily range of 8 deg.; the minimum being 40 deg., the maximum, 80 deg. During the rains, the mean temperature is 63 deg., with a range of 3 deg. From March, till the bursting of the monsoon in June, the climate is mild and genial, with warm days and cool pleasant nights. The annual rainfall is 253.24 inches, of which 45.52, 97.88, 68.86, and 31.48 inches were registered in June, July, August, and September. The cause of this great rainfall is the striking of the south-west monsoon against the outlying range of the Ghats, and the deposition on their heights and slopes of the greater portion of its aqueous vapour. During the periodical rains, the visitors of all denominations retire *en masse* to Poona. The season reopens in October, when the mean temperature of the month is 66 deg., with a range of 7 deg. In November it is 62 deg.; and in winter, the greatest cold is accompanied only by slight morning hoar frosts, due chiefly to rapid nocturnal radiation and evaporation. During this season most of the purely functional effects of malarial disease and heat are benefited. But, after the mists set in in May, it is, in spite of the remarkable equableness of the temperature, regarded as prejudicial to persons suffering from phthisis, rheumatism, confirmed liver, bowel and heart affections.

18. *The Nilgherries.*—This, the Queen of Indian sanitaria, lying between 11 deg. 12 min. and 11 deg. 37 min. latitude N., and 76 deg. 18 min. and 77 deg. 5 min. longitude E., extends from north to south twenty-nine miles, and from east to west fifty-one miles. The average elevation is about 6,500 feet; that of the Wynaad district, now added to the Nilgiri Commissionership, is about 3,000 feet. There are

four chief sanitaria on these hills, viz., Ootacamund or Ooty, Coonoor, Wellington, and Kotagiri.

(A.) *Ootacamund.*—This station has an elevation of 7,000 feet. It is situated on the slopes, and at the foot of Dodabetta (8,622), Church hill (7,429), Snowdon (8,299), and Club hill (8,030 ft.), and is in part divided by a serpentine lake about a mile and a quarter long, which is surrounded by an excellent carriage road, upwards of eight miles in length. The mean annual temperature is 58.69 deg., or about 1 deg. below that of Nice. In the warmest months, the temperature rarely surpasses 75 deg. in the shade, occasionally descending to 34 deg. During this period the average range is about 9 deg. In December and January, frost sometimes occurs in the valleys, and the range is 16 deg. The rainfall is 44.8 inches. The main force of the monsoon is expended on the Khoondah mountains to the south-west. The south-west monsoon often opens with violent gales, and, as the surrounding heights are for long periods enshrouded in mist and fog, the atmosphere is raw and moist, and the climate somewhat depressing. During the winter the air is comparatively dry and bracing, but, owing to rapid radiation, invalids have to protect themselves by clothing very warmly, to mitigate the sudden lowering of the temperature following sunset.

(B.) *Coonoor.*—This station is twelve miles from Ootacamund, 6,500 feet above the level of the sea, and situated at the south-east corner of the Nilgherry plateau. The mean annual temperature is 64 deg., and that of the summer 70 deg., descending to 60 deg. at night. It is, therefore, warmer than Ootacamund. The rainfall is 55 inches. This comparatively small measure being due to the interception of the weight of the monsoon rain, first by the Khoondahs, and, secondly, by the higher mountains in the neighbourhood of Ooty. It is probably more equable than Ooty, owing to its lower elevation, to its being shielded by the Dodabetta mountain to the north, and to its being directly influenced by the south-west monsoon from June to September, and by the north-east monsoon from October to December.

(C.) *Wellington.*—This is a military sanitarium, a mile and a half from Coonoor, 6,200 feet above the level of the sea, and situated on the Jackatalla plateau. Being somewhat lower than Coonoor, and equally sheltered, the climate is proportionately milder.

(D.) *Kotagiri.*—This station, imperfectly developed, chiefly on account of its comparative inaccessibility from the plains, is situated on the north-east end of the Nilgherry hills, nine

miles from Coonoor, and fourteen miles from Ootacamund. It is 6,500 feet above the level of the sea. It comes under the direct influence of the north-east monsoon more than Coonoor. The result of both monsoons is the production of a rainfall of 50 inches. It is the most equable of all the Nilgherry sanitaria, and was held in the highest esteem by the late Marquis of Dalhousie.

It will be apparent from the foregoing remarks that, in the Nilgherries, the invalid may exercise a choice of climate not presented in anything like an equal degree by any of the other sanitaria brought under notice in this communication. At the beginning of the hot weather in March he may select Coonoor for its mildness, warmth, and equability for a short sojourn to prepare his constitution for the more elevated, cooler, and more bracing climate of Ootacamund; and should he resolve to remain in these hills after the south-west monsoon has commenced, he may, if he can secure accommodation, go to Kotagiri, where the inclemency of this season is only experienced in a minor degree. And, again, to avoid the north-east monsoon at Kotagiri, he may, in the beginning of October, exchange residence there, during that and the remaining winter months, for Ootacamund, where its force is scarcely, if at all, felt.

The general experience of the able medical officers who have held medical charge of these sanitaria, tends to the conclusion that whilst they are, at appropriate seasons, eminently useful and restorative in all cases of ill health caused by the enervating heat and malaria of the plains, and consequent functional derangements of the nervous, digestive and respiratory systems, they are not advantageous to those suffering from liver enlargement and congestion, dysentery, or diarrhœa, chronic bronchitis, asthma, phthisis, and cardiac disease. Hill diarrhœa, or the diarrhœa alba, such as is common at Simla, is not a frequent drawback at any of the Nilgherry sanitaria. The climate proves very healthy to children free from tuberculous taint.

19. *Newera Eliya* (Ceylon).—As the eastern and western aspects of the island bear a strong floral and geological resemblance to the corresponding faces of the neighbouring continent, the chief sanitarium of Ceylon may be appropriately included in this concise summary of the leading medico-meteorological characteristics of the hill climates of India. It is 6,150 feet above the sea, and 7 deg. north of the equator. The mean annual barometric pressure is 24,069 deg. It varies but little throughout the year; the minimum reading being 24.41 deg. in July; the maximum being 24.099

deg. in March. The mean annual temperature is 59.3 deg. The lowest mean monthly temperature is 57.7 in December; the highest 61.2 in May. Frost is understood to be unknown. The mean yearly daily range is 17.2 deg. The lowest mean monthly range is 10.9 deg. in September; the highest 24.9 deg. in March. The absolute range is 45.5 deg. The annual rainfall is 99.45 deg. The station is often for days and weeks together under a canopy of clouds, emptying themselves in torrents of rain, in every month of the year excepting January and February. The humidity is great almost throughout the year. Add to this a too extensive daily range between the heat of night and day, and we have a climate unsuited to cases of phthisis, hepatic and bowel affections, rheumatism and diseases of the heart. It is, however, protective against malarial fevers and dyspepsia in permanent residents. It is also beneficial to the fever-stricken denizens of miasmatic districts, provided they have not contracted organic complications. But its value in this respect is far inferior to the Nilgherry and Himalayan sanitaria.

By the aid of the foregoing facts, the ground has, in some measure, been cleared for a critical consideration of the value of the hill stations of India and Ceylon in cases of scrofula, tuberculosis, and phthisis. The true bearings of the subject will, perhaps, be best dealt with by making an effort to estimate the influence of these sanitaria (1) upon persons who have inherited or acquired the scrofulous diathesis; (2) upon those who have been diagnosed, after careful scrutiny, to be the subjects of tubercular growth; and (3) upon those who are affected with confirmed consumption or phthisis.

1. *The Influence of Residence at Hill Stations upon those who have Inherited or Acquired the Scrofulous Diathesis.*—It may be assumed as proved, that almost all the hill sanitaria enumerated in this paper are sufficiently elevated, temperate, and drained, to be free from the generation of malaria. They are all above 3,000 feet in height, the altitude, according to Parkes, beyond which, in India, the malarial poison, germ, or bacteria, as the case may be, does not appear to be reproduced. Permanent residents of all races enjoy immunity from paroxysmal fevers. Persons suffering from these diseases contracted on the plains, provided no enduring organic mischief has resulted, are cured by residence in these climates. When recurrent attacks do occur, there is no need to assign them to malaria generated on the spot. There would seem to be good reason for believing that this tendency to repetition, characteristic of this class of fevers, especially on exposure to damp, vicissitudes of temperature,

and great privation, does not always of necessity imply a fresh imbibition of malarial poison. It often happens that individuals who have never had ague at all, or, having had it, have not been attacked with it for many years, are, after having gone to the hills, or returned to this country from malarious districts, liable to suffer from primary or secondary paroxysms. Then, in all hill stations, according to altitude, latitude, humidity, and other agencies, the temperature in the shade is seldom extreme and exhausting during the summer, and, though in most of them the rainfall is considerable, in only a very few—to wit, Cherrapoonjee and Mahableshwur—is it ever so continuously excessive as to prohibit at some period of nearly every day daily open-air exercise. During the hot months, in all the stations of the higher altitudes—Simla, Mussoorie, Darjeeling, and the Nilgherries, etc.—it is always needful to have the head thoroughly shielded from the direct rays of the sun; and at all those of lower elevation, such as Pachmari, Chikalda, and Mount Aboo, the same precaution must be enforced during the winter months. In all these sanitaria, the cold season—though in some, and especially at those situated on the Indian aspect of the Himalayas, accompanied by ice, frost, and snow—proves to those in moderately fair health, and in a position to be much out in the open air, bracing, exhilarating, and salubrious. The thermometric range in the cold and hot weather, though greater than would be beneficial in organic disease of any of the prime organs, is not sufficiently extreme to warrant an adverse opinion of these climates in the case of persons who are regarded as labouring under the taint of the scrofulous diathesis. Their elevation above the sea necessitates increased expansion of the chest-walls and of the pulmonary cells, in order that the required amount of oxygen may gain free and uninterrupted access to the blood. At these altitudes, the air is attenuated, or rarefied as we say; and that the blood circulating through the lungs may be provided with a given quantity of oxygen to meet the physiological demands of the system, a greater volume must be passed into these organs at each inspiration. In accurate response to this necessity, the reserve expansibility of the thorax and pulmonary cells is speedily developed. Whether, *cæteris paribus*, more oxygen actually enters the lungs in higher than in lower altitudes has not, so far as my knowledge extends, been sufficiently determined. If a comparison be made, however, between these temperate elevations and the lower level of the plains of India, the legitimate presumption would be that, as residence at these hill sanitaria is con-

joined with the capability of enjoying augmented physical exercise, the supply of oxygen would be proportionately increased. Thus, the increased tone and power of the respiratory and other muscles throughout the body, of the air-cells of the lungs, and of the general and special nervous systems, accompanied by improvement of the appetite, and the primary and secondary processes of assimilation, cannot, under the circumstances mentioned, be otherwise than beneficial to the inhabitants and visitors of the mountain climates of India. In addition to these advantages, the scrofula-breeding diseases of childhood and youth are not so prevalent as they are in other places out of India. Thus, it may be concluded that residence at most of the hill stations is probably conducive to the promotion and conservation of health in most persons who may have inherited or acquired the taint of scrofula. With the exception of Mahableshwur and Cherrapoonjee, they may live at these stations with a good chance of deriving some benefit all the year round. If the monsoon season appear to be prejudicial to some in the Himalayan sanitaria, the effect of this can, in the manner already pointed out, be modified or mitigated by selecting the Nilgherry mountains as the place of temporary or permanent residence.

2. *The Influence of Residence at Hill Stations upon those who are the Subjects of Tubercular Growth.*—Provided there is no pneumonic inflammation in the vicinity of the growth at the upper part of the lungs, and there has been no prior structural disintegration, the summer season, up to the beginning of the rains, in all the sanitaria may be viewed as beneficial. The expansion of the chest and lungs from altitude and the rarefaction of the air, the increased power of taking exercise in the open air, are advantageous in a high degree. None of them, in my experience, are suitable in the rainy season. The small range then prevailing is more than neutralised by the extreme humidity of the air. Persons coming under this heading bear badly the increased thermometric range in the winter months. By way of interpolation, it may here be mentioned that for cases of simple tubercular growth, or the taint of scrofula, a voyage to India to escape the English winter may often prove far more serviceable than any of the marine sanitaria on either of the shores of the Mediterranean. The long sea voyage there and back, in any of the comfortable steamers now available, in mild, warm, and genial weather, amid ever-changing surroundings and associations, and, after arrival, easy travelling to visit places of historic interest, are calculated to improve and preserve the general health. The journey should be commenced

in October; arrival in India timed to be in November; re-embarkation from Calcutta or Bombay about the middle—not later than the end—of March. The time intervening between the arrival in Europe and June might be profitably passed in the North of Italy and on the way home slowly, so that these islands may not be reached until the beginning of June.

3. *The Influence of Indian Hill Climates upon those who are Affected with Confirmed Consumption or Phthisis.*—There is an almost universal consensus of opinion among the British and Indian medical officers, condemnatory of hill stations in cases of pulmonary consumption. From the earliest times at which soldiers were sent to the hills with phthisis, it was noted that the disease became aggravated by the change, and the end expedited. The rule has long been, and now is—practically amounting to an authoritative order—to avoid the transfer of all invalids of the European army in India, and others of the various services in that country afflicted with phthisis, to any of the hill sanitaria. A few years ago, on reports being received of the favourable influence of elevated regions in some parts of Europe in consumption, some physicians, including myself, ventured to advise patients to try Coonoor in the Nilgherries. In every case in which I was directly or indirectly interested, the result was extremely disappointing. The reason of this is not difficult to divine. Healthy and invigorating as most of the Indian sanitaria are for the scrofulous during, perhaps, the entire annual cycle of the seasons, and for those suffering from tubercular growth in a comparatively quiescent condition during the summer, or about three months of the year, they are just the reverse in confirmed phthisis. Here, instead of our only having the scrofulous diathesis or tubercular deposit in a somewhat inactive state to deal with, we have superadded acute, or chronic and more or less persistent pneumonic and bronchial inflammation tending to, and eventually resulting in, ulcerative disintegration of the structures immediately involved. Now, in a case of this kind, the earliest effect of a high altitude—and this effect, *cæteris paribus*, will be in proportion to the amount of the elevation above the sea level—is to cause the inflamed, irritable, and diseased lung to do more work than it was, at lower levels, called upon to perform. The increased expansion of the air-cells is needed to allow of a sufficient ingress of oxygen in a given period of time into the pulmonary circulation from a thin, attenuated, and rarefied atmosphere. The consequence is, in my experience, to increase irritation and inflammation

around the tubercular growths, and the tendency to repeated attacks of hæmoptysis. According to Dr. Theodore Williams, the circumference of the chest has been known to have increased three inches during the season at Davos. A lung in the condition above mentioned surely does not require more movement than can be averted. This is not the guiding principle of the surgeon in his management of injuries—inflammatory or otherwise. Nature points the way, and he, in ready compliance with her dictates and teaching, is constantly invoking the aid of his best ally—physiological rest. Again, what does the physician do in pleurisy, pericarditis, or typhoid fever? Why, both in the food and medicine employed, and the repose enforced, he conforms to this great principle of as much comfort and rest as can be commanded for the diseased parts; and if it were possible to use mechanical means and appliances to accomplish this, he most probably would not hesitate to have recourse to them.

Doubtless there are cases of chronic tubercular affection of the lungs in which the inflammatory action previously existing has subsided, and the patients are in much the same condition as those who are only the subjects of simple tubercular consolidation. There is another rarer set of cases, in which cicatrisation or cretification has proceeded to establish the repair of the damaged lung. In such cases as these, hill stations are suitable and advantageous at certain seasons, not so much on account of the greater expansion of air-cells of the lungs, as from the immensely augmented open-air life, and moderate exercise available in a mild, equable, pure, and antiseptic atmosphere.

My conclusions are, in a summary form, (1) that the hill sanitaria of India are beneficial to persons afflicted with scrofula only; (2) that they are useful, in the warm months of summer, (*a*) in uncomplicated and simple tubercular consolidation, (*b*) in tubercular affection, in which previous inflammation of the adjacent lung and the bronchial mucous membrane has subsided, and (*c*) where this is proceeding in the direction of repair by absorption, cicatrisation, or cretification; and (3) that they are injurious, probably at all seasons, but in an aggravated degree in the cold season and rains, in all cases of active and advancing pulmonary consumption.

APPENDIX,

Consisting of a series of tabular statements showing the latitude, longitude, elevation, barometric or air pressure, mean daily range, absolute range and rainfall at Leh, Murree, Simla, Mussoorie, Ranikhet, Chakrata, Darjeeling, Cherrapoonjee, Mount Aboo, Pachmari, Chikalda and Newera Eliya (Ceylon); compiled from the Report on the Meteorology of India in 1878, by Henry F. Blanford, F.R.S.

TABLE I.—LEH.

Latitude, North, 34 deg. 10 min.; Longitude, East, 77 deg. 42 min.; Elevation, 11 538 feet.

Months.	Barometric or Air pressure.	Mean Daily Temperature.	Mean Daily Range.	Absolute Range.	Rainfall in inches.
January ...	19.608	6.2	36.6	59.9	0.10
February ...	19.588	15.3	33.1	55.9	0.01
March ...	19.679	28.0	28.5	60.5	0.01
April ...	19.638	38.9	25.1	40.9	nil
May	19.670	44.8	27.1	42.5	0.05
June ...	19.660	52.8	29.6	47.5	0.03
July	19.617	59.7	28.7	46.9	0.20
August ...	19.643	60.5	29.0	44 8	0.71
September ...	19.703	53.8	30.8	44.8	0.21
October ...	19.725	41.4	30.6	45.7	nil
November ...	19.723	31 6	31.3	40.9	nil
December ...	19.695	22.3	31.6	42.8	nil
Year ...	19 662	37.9	30.2	103.6	1.32

TABLE II.—MURREE.

Latitude, North, 33 deg. 40 min.; Longitude, East, 73 deg. 8 min.; Elevation, 7,507 feet.

January ...	23.891	36.7	12.6	34.9	0.30
February ...	23.851	37.9	11.1	30.3	3.44
March ...	23.854	50.7	15.0	34.3	2.20
April ...	23.867	54.0	14.6	36.7	5.08
May	23.805	59.0	14.1	33.9	4.25
June ...	23.759	71.0	16.7	43.9	1.93
July	23.737	69.3	13.9	38.6	12.45
August ...	23.774	66.4	11.7	23.6	15.09
September ...	23.845	66.4	15.5	29.2	3.98
October ...	23.929	59.6	17.5	33.7	4.36
November ...	23.958	52.0	15.4	29.1	3.53
December ...	23 932	45.2	15.3	36.5	1.83
Year ...	23.850	55.7	14.3	72.7	58.44

TABLE III.—SIMLA.

Latitude, North, 31 deg. 6 min.; Longitude, East, 77 deg. 12 min.; Elevation, 6,952.74 feet.

Months.	Barometric or Air pressure.	Mean Daily Temperature.	Mean Daily Range.	Absolute Range.	Rainfall in inches.
January ...	?	39.3	17.7	36.0	2.04
February ...	23.255	39.9	16.8	33.0	2.80
March ...	23.268	49.9	...	...	3.36
April ...	23.242	51.6	24.1	47.7	3.39
May	23.202	...	...	...	4.56
June ...	23.140	...	...	...	7.90
July	23.138	65.5	15.2	26.9	19.36
August ...	23.200	63.5	10.8	17.9	18.17
September ...	23.262	62.5	16.0	27.9	6.27
October ...	23.348	57.7	17.5	27.8	1.21
November ...	23.363	52.1	20.2	32.8	0.17
December ...	?	...	19.7	30.7	0.97
Year ...	?	...	...	...	70.20

TABLE IV.—MUSSOORIE.

Latitude, North, 30 deg. 27 min. 30 sec.; Longitude, East, 76 deg. 6 min. 30 sec.; Elevation, 7,433 feet.

January ...	...	42.1	12.4	25.5	2.15
February ...	...	46.5	12.8	27.4	3.34
March ...	...	57.2	14.0	36.3	3.18
April ...	...	60.3	14.4	38.3	2.05
May	...	60.7	13.6	31.2	2.86
June ...	...	74.2	16.0	35.3	9.43
July	...	67.1	10.3	19.1	29.68
August ...	...	65.7	8.5	18.0	30.75
September ...	...	65.3	10.1	22.1	9.47
October ...	...	63.0	13.6	21.1	1.09
November ...	...	56.6	16.3	25.2	0.10
December ...	...	...	...	...	1.11
Year ...	...	...	...	...	95.21

TABLE V.—RANIKHET.

Latitude, North, 29 deg. 38 min.; Longitude, East, 79 deg. 29 min.; Elevation, 6,068.75 feet.

January ...	24.106	44.0	14.0	27.7	1.83
February ...	24.087	48.6	11.9	28.7	2.68
March ...	24.074	57.5	15.6	39.5	1.59
April ...	24.078	60.7	15.1	35.2	1.94
May	24.021	60.6	14.9	32.0	3.46
June ...	23.951	74.5	14.6	28.6	5.32
July	23.938	68.2	10.8	21.5	12.69
August ...	23.969	67.2	9.3	17.5	10.84
September ...	24.035	67.0	11.3	17.7	5.44
October ...	24.115	63.4	14.5	24.0	0.58
November ...	24.171	57.6	14.6	25.6	0.09
December ...	24.148	49.3	15.0	26.0	1.17
Year ...	- 24.058	59.9	13.5	56.6	47.63

TABLE VI.—CHAKRATA.

Latitude, North, 30 deg. 40 min.; Longitude, East, 77 deg. 55 min.; Elevation, 7,051.58 feet.

Months.	Barometric or Air pressure.	Mean Daily Temperature.	Mean Daily Range.	Absolute Range.	Rainfall in inches
January ...	23.263	39.6	14.2	31.7	1.35
February ...	23.224	42.8	13.8	30.6	2.17
March ...	23.244	53.5	17.4	40.7	3.52
April ...	23.231	55.4	16.1	41.3	2.05
May	23.193	55.7	14.9	30.2	3 39
June ...	23.154	70.2	16.1	27.2	7.03
July	23.121	64.4	11.4	21.5	17.36
August ...	23.170	63.6	9.6	19.4	15.18
September ...	23.229	63.4	11.3	21.0	5.89
October ...	23.293	59.5	14.3	23.5	0.67
November ...	23.313	54.2	15.3	26.1	0.15
December ...	23.294	45.7	17.0	28.4	1.20
Year ...	23.227	55.7	14.3	59.7	59.96

TABLE VII.—DARJEELING.

Latitude, North, 27 deg. 3 min.; Longitude, East, 85 deg. 18 min.; Elevation, 6,912 feet.

January ...	23.396	39.2	13.6	26.3	0.72
February ...	23.376	44.7	12.6	30.4	1.24
March ...	23.375	50.4	17.2	35.9	2.30
April ...	23.374	55.2	15.7	30.9	3.97
May	23.342	56.6	14.2	26.0	6.41
June ...	23.284	63.3	12.9	27.7	23.97
July	23.276	63.2	10.6	17.0	29.80
August ...	23.320	63.1	10.6	21.1	25.53
September ...	23.377	62.3	10.4	18.6	17.53
October ...	23.440	58.6	15.8	24.8	6.36
November ...	23.473	52.7	15.3	25.4	0.23
December ...	23.448	45.3	16.8	26.9	0.18
Year ...	23.373	54.6	13.8	53.0	118.24

TABLE VIII.—CHERRAPOONJEE.

Latitude, North, 25 deg. 15 min. 58 sec.; Longitude, East, 91 deg. 46 min. 42 sec.; Elevation, 4,588 feet.

January ...	.....	.....	.....	.....	0.73
February ...	.....	.....	.. ...	.....	2.76
March ...	.....	.....	.....	.....	7.05
April ...	.....	.....	.....	.....	30.63
May	.....	.....	.....	.....	51.09
June ...	.....	.....	.....	.....	114.70
July	.. ...	.....	.....	.....	133.54
August ..	.....	.....	.....	.....	77.31
September ...	.....	.....	.....	.....	58.01
October ...	.....	.....	.....	.....	14.02
November ...	.....	.....	.....	.....	2.50
December ...	.....	.....	.....	.....	0.11
Year ...	.....	.....	.....	.....	492.45

TABLE IX.—MOUNT ABOO.

Latitude, North, 24 deg. 36 min.; Longitude, 72 deg. 45 min.; Elevation, 3,945 feet.

Months.	Barometric or Air pressure.	Mean Daily Temperature.	Mean Daily Range.	Absolute Range.	Rainfall in inches.
January ...	?	54.2	20.1	43.9	0.19
February ...	?	62.6	16.4	35.1	0.34
March ...	?	72.5	18.3	43.0	0.17
April ...	?	74.5	17.2	35.8	0.05
May	25.969	77.9	16.7	28.7	1.02
June ...	25.882	77.2	16.1	30.1	5.43
July	25.817	72.0	9.6	19.5	20.63
August ...	25.860	69.2	6.9	16.2	23.12
September ...	25.954	71.7	10.7	20.7	8.47
October ...	26.081	73.5	15.0	25.7	2.46
November ...	26.110	65.3	18.1	31.1	0.22
December ...	26.118	36.6	19.1	33.6	0.26
Year ...	?	69.0	15.4	61.9	63.36

TABLE X.—PACHMARI.

Latitude, North, 22 deg. 28 min.; Longitude, East, 78 deg. 28 min.; Elevation, 3,504 feet.

January ...	26.539	59.4	20.7	46.7	0.72
February ...	26.538	66.3	21.4	41.9	0.43
March ...	26.491	73.0	25.8	55.8	0.31
April ...	26.413	78.5	21.8	37.5	0.44
May	26.344	81.8	21.0	33 0	0.55
June ...	26.351	83.1	16.2	31.4	9.86
July	26.218	73.2	8.4	18.2	30.99
August ...	26.267	71.4	7.4	14.8	20.09
September ...	26.332	72 0	10.1	21.1	16.00
October ...	26.472	71.4	15.5	30.5	1.03
November ...	26.537	63.5	21.3	39.6	0.04
December ...	26.551	54.7	26.4	44.9	0.47
Year ...	26.413	70.7	18.0	71.5	80.93

TABLE XI.— CHIKALDA.

Latitude, North, 21 deg. 24 min.; Longitude, East, 77 deg. 22 min.; Elevation, 3,656 feet.

January ...	?	64.4	15.8	32.7	0.82
February ...	?	70.5	18.1	34.4	0.13
March ...	?	77.4	18.8	44.0	0.46
April ...	?	79.9	19.5	32.5	0.16
May	?	81.8	20.3	34.8	0.17
June ...	?	79.6	17.9	31.8	8.89
July	?	70.2	7.7	19.8	15.12
August ..	?	69.5	7.3	13.4	16.73
September ...	?	70.1	8.6	14.7	11.17
October ...	?	70.4	11.4	19.5	3.73
November ...	?	67.7	13.8	23.0	0.07
December ...	26.426	62.0	16.4	32.1	0.68
Year ...	?	72.0	14.6	24.0	58.13

TABLE XII.—NEWERA ELIYA.

Latitude, North, 70 deg.; Longitude, East, 80 deg. 42 min.; Elevation, 6,150 feet.

Months.	Barometric or Air pressure.	Mean Daily Temperature.	Mean Daily Range.	Absolute Range.	Rainfall in inches.
January ...	24.086	59.4	13.2	27.5	6.81
February ...	24.094	57.8	23.2	42.0	2.08
March ...	24.099	57.9	24.9	41.5	2.59
April ...	24.070	59.0	24.3	39.0	6.51
May	24.063	61.2	18.6	29.0	8.15
June ...	24.047	59.2	11.8	24.3	16.36
July	24.041	59.7	15.2	29.0	12.52
August ...	24.042	60.3	12.0	24.0	7.26
September ...	24.057	60.0	10.9	18.5	10.65
October ...	24.063	60.1	16.0	25.3	10.19
November ..	24.085	58.7	17.7	35.8	9.49
December ...	24.082	57.7	18.7	33 5	6.84
Year ...	24.069	59 3	17.2	45.5	99.45

ON THE PROBABILITY THAT THE INFECTION OF DIPHTHERIA IS SOMETIMES TRANSPORTED BY THE WIND.

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(*Read:* 1*st March*, 1881.)

Some recent decisions of the law courts affecting the legal position of small-pox hospitals have given a very practical interest to the investigation of the question whether the infection of small-pox is conveyed by the outer air. And it is not unreasonable to expect that the Royal Commission on Hospitals, now sitting and taking evidence, will in its forthcoming report be able to disclose important facts bearing upon that question, perhaps fully solving it, or leading to its sure and speedy solution. For us, the interest of such investigation does not limit itself to small-pox alone, but must extend to the generality of infectious diseases. Can the infection of cholera, enteric fever, erysipelas, scarlatina, diphtheria, be conveyed by the air? If any such facility of transportation should be established in respect of small-pox, it will become necessary to scrutinise, by that light, and from that point of view, the behaviour of every other infection, to see if they do not all, or many of them, enjoy, in their degree, a like freedom and ease of dissemination.

Early in 1880, some time before this question came to the front in regard of small-pox, my attention had been drawn to the possibility of aërial conveyance of the infection of diphtheria, by a short report to the Bideford Rural Sanitary Authority, in which Mr. Wynter Blyth, who had been Medical Officer of Health for that district, gave an account of an outbreak of diphtheria (the facts of which subsequently came within my own knowledge) at Woolfardisworthy, near Hartland, in North Devon. The case was a typical one. Diphtheria broke out, at the end of August 1879, in a family of children living at an isolated farmhouse on the summit of a hill nearly two miles distant from the village. Six children in this family were attacked with the disease, the first five not severely, the sixth in a malignant and fatal form.

No previous diphtheria could be traced in the neighbourhood within four miles, nor at that distance within three months. Nor could any train of personal communication be traced, or reasonably suspected, between this isolated farmhouse and any previously infected place. The drinking-water was good, and the general sanitary condition of the place satisfactory. It was a case typical of many similar cases, in which the inquirer was driven to one of two suppositions. Assuming the disease to be incapable of spontaneous origination in the human frame, it seemed that he must look either to some local miasm or to some distant centre of infection for the cause of the outbreak. The lofty breezy situation of the farmhouse emphatically negatived the notion of a local miasm, while it as strongly suggested the idea that, if the infection of diphtheria were conveyable from some distant source by the wing of the wind, this would be the very spot where its effects might most probably be expected to show themselves. For five or six days before the outbreak, the wind was blowing strongly from the south-west; and in that direction lies the Kilkhampton sub-district, in which a death from diphtheria was registered during the third quarter of that year, but while on the spot I did not hear of diphtheria in that direction. I am giving this description only by way of showing the sort of conditions and circumstances under which the idea of aërial infection is first suggested and begins to commend itself to the mind.

The idea becomes still more familiar when it is even more suggestively presented by the next instance the observer chances to meet with. In April 1880 diphtheria broke out, with no traceable history of personal infection, and no suspicious local conditions, at a small farmhouse at a considerable height on the north side of Ashdown Forest, near East Grinstead, in Sussex. The direction of the wind had been easterly; and in that direction, in the Tunbridge district, there had been eighteen deaths from diphtheria in the year 1879, and two in the first quarter of 1880. Thus far the circumstances were somewhat similar to those at Woolfardisworthy; but, in this outbreak near East Grinstead, there were additional facts of a secondary character, which appeared to strengthen the evidence of conveyance of the infection by the wind. About a fortnight after the last fatal case at the farmhouse, a young man was taken ill at a cottage on a high ridge, about a mile to the westward, separated by a deep hollow from the spur on which the farmhouse stood. Here, again, I could find no ground for suspecting any per-

sonal communication with the infected farmhouse or household. The sanitary condition of the cottage was good, and no relation with the earlier cases could be plausibly suggested, except that the wind had been blowing from that quarter about a week before this young man fell ill.

Subsequently, diphtheria got among the children in the village of Forest Row, at the foot of the spur on which the last-mentioned cottage stands. (This extension was probably due to the infection being brought into school by an infected family living near the farmhouse where the first outbreak occurred.)

Still later (in August), the disease appeared at the village of West Hoathly, on a hill-top a little more than four miles to the west-south-west from Forest Row. A valley leads straight in that direction to the foot of the hill on which the village stands, and then turns to the north-north-west. A north-east wind had been blowing for five days before the outbreak at West Hoathly. Such a wind would, of necessity, press with increased velocity over the crest of the hill that barred its way; and the theory is suggested, in default of any traceable chain of personal communication, that infective particles were wafted from Forest Row to West Hoathly by the wind, and there caused diphtheria in those by whom they were inhaled.

These instances I bring forward as examples, at once, of the obscurity, or rather total darkness, that enshrouds the origin of most diphtheria outbreaks, on any severe hypothesis of personal contagion, and also as examples of the way in which topographical features will sometimes hint aërial infection, and of the facility with which that theory will explain the most puzzling facts of diphtheria extension.

For accounts of other typical instances I would refer to Mr. Wynter Blyth's paper on "The Prevention and Propagation of Diphtheria", which is published in the *Sanitary Record* for May 1880, p. 407; and Dr. Edwyn Slade King's "Notes on the Spread of Diphtheria" (read, I believe, at the Exeter meeting of the Sanitary Institute, in September 1880).

In the *New York Medical Record* for January 22, 1881, vol. xix, p. 92, there is an account, by J. W. Pinkham, M.D., of an outbreak of diphtheria in a house of which one part was warmed by a Gould's hot-air furnace. Twenty-seven feet from the cold-air box was an offensive stable-and-kitchen midden. Before the outbreak, the wind was setting from the refuse-heap towards the house. Eight persons slept in the part of the house that was warmed by the hot air, and five of them were attacked with diphtheria (three fatally) in the

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course of seven days. The remaining members of the household, five in number, who occupied rooms not so warmed, all escaped.

Corroborative testimony on a large scale, though expressed in vague and general terms, comes from Russia, where in recent years, if we may believe the paragraphs that have appeared in the London press, the ravages of diphtheria have been appalling. In the *Daily News* of September 14, 1881, it was stated that in certain communes and parishes this disease was reported to have carried off all the children up to fifteen years of age. "From the official statistics of the Medical Department, it appears that the origin of the attack dates from 1872, when it made its first appearance in Bessarabia; since then, it has spread far and wide over the southern part of the empire, whence it has lately begun to make rapid progress towards the east and north-west. *It is observed that it follows the direction of the prevailing winds of the southern portion of Russia in Europe.* . . . In Pultowa, a province of considerably less than 2,000,000 inhabitants, there have been 45,543 cases, of which no fewer than 18,765 have proved fatal."

All the above-mentioned instances and suggestions, put together, only serve, at the utmost, to make out a case for further and more accurate inquiry. They are all selected instances, and I need hardly say that no safe conclusion can be formed from data thus gathered. But I think it will be admitted that the facts here brought forward, selected though they be, are sufficiently striking and suggestive to make it worth our while to look a little closer into the question. We require to take a large number of instances, without any such *ex parte* selection as would vitiate the results, and examine them individually and collectively, and see whether they give a resultant testimony for or against the theory.

Selection, in some measure, there must be, in order to exclude certain manifest sources of error. For example, it would be out of the question to include cases in which there was uncertainty about the date of first attack; and it would be unreasonable to include cases in which there was sufficient evidence of importation of the disease, or of personal communication with some previously infected place, or of exposure to effluvia from untrapped and unventilated drains, or other satisfactory means of accounting for the origin of the outbreak. It is not with such cases that the theory of aërial infection is concerned. It seeks only to explain those inscrutable occurrences of epidemic or of so-called sporadic diphtheria in which other recognised lines of investigation

fail altogether, and leave the inquirer utterly at a loss where to turn for a clue to lead him out of the labyrinth.

Accordingly, the outbreaks suitable for analysis will be all, without exception, that fulfil the following conditions:—1. The date of first attack must be exactly known. 2. The observer must be satisfied that the first reputed case of diphtheria was not really preceded by cases of apparently simple sore-throat in the same locality. 3. He must be satisfied that the outbreak could not be explained by reference to any of the known modes of diphtheria spread.

Should the general results of a first analysis be found to give countenance to the theory, we might afterwards try to define more precisely the conditions under which the theory was properly applicable. For example, we might ask whether there was not ground for suspecting that in certain places, at a certain season of the year, diphtheria could arise independently of any previous case of the disease, in virtue of the presence of some local malaria comparable to that which causes ague; and if so, we might recognise a class of original cases, which should show no dependence upon wind-conditions, though they might be called veritable cases of aërial infection. Again, we might question whether the distinction which has been observed between rural and urban diphtheria did not justify us in separating them accordingly into two classes, for purposes of comparative analysis.

These, however, are prospective refinements which it would be premature now to dwell upon. For the present, it will be sufficient to observe the three limitations indicated above. Keeping these in view, I have now to ask the Society to take into consideration the evidence which I brought before the International Medical Congress in August of last year. My excuse for reproducing matter which has been already made public must be my anxiety to obtain the opinion and judgment of this Society upon the value of the evidence and the validity of the inferences to be drawn from it.*

The following is the list of outbreaks, thirty in number, which I have submitted to analysis in respect of the wind-conditions preceding the dates of the first attack. They are gathered from reports of the medical inspectors of the Local Government Board, ranging from 1875 to 1880.

* I owe it to the courtesy of the publisher of the *Transactions of the International Medical Congress*, 1881—Mr. J. W. Kolckmann of London—that I am able to reproduce from that publication the chief of the diagrams which illustrated my paper.

Place of Outbreak.	Date of First Attack.	Probable Date of Infection.	Direction of Wind.	Previously Infected Place.	Distance in Miles.
	1875.				
1. Coggeshall, Essex	Nov. 11	Nov. 6	W. and W.S.W.	Braintree or Black Notley	6
2. South Ash, Kent	Dec. 27	Dec. 20-24	S.W., S.S.W., and W.S.W.	Sevenoaks subdistrict	9
3. Stokesay, Shrops.	Dec. 24 1876.	Dec. 19, 20	S. and S.W.	Onibury ...	3
4. Bromfield, Shrop.	Feb. 3	Jan. 29-31	S.E., S., S.S.E.	Ludlow? ...	3
5. Penkridge, Staff.	Feb. 14	Feb. 6-8	N., N.N.E.	Stafford ...	6
6. Brailes, Warwickshire	May 6	April 30-May 3	N.E., N.N.E.	Tysoe ...	5
7. Padstow, Cornw.	June 18	June 14, 15	S.S.W.	New Quay ...	12
8. Radwinter, Essex	June 22 1877.	June 15, 16	S.E.	Coggeshall?	18
9. Anlaby, Yorksh.	April 24	April 16-18	E., E.N.E., N.E.	Hull	3
10. Swanland, Yorks.	May 11	May 4-6	E., E.N.E., N.E.	Hull	6
11. Denbigh... ...	Aug. 10.	Aug. 2-4	W.N.W.	Llanfairtalhaiarn	8
12. Wrexham, Denbighshire	Nov. 23 1878.	Nov. 18-20	N.W., W.N.W.	Brymbo ...	3½
13. Cranfield, Beds.	June 26	June 14, 15	N.E., N.N.E.	Bedford? ...	7
14. Bleadon, Somersetshire	Oct. 10	Oct. 5-7	S.S.E.	Wedmore ...	8
15. Staple Cross, Sussex	Nov. 4 1879.	Oct. 28-30	W.N.W.	Rotherfield ...	15
16. Swanscombe, Knt.	July 26	July 21, 22	N.W.	Barking ...	12
17. Trotterscliffe, „	Sept. 15	Sept. 8-12	S.W.	Shoreham subdistrict?	6
18. Felstead, Essex	Sept. 12	Sept. 7-9	S.W.	London? ...	30
19. Henley, Suffolk	Nov. 21	Nov. 12, 13	N.W.	Needham ...	4
20. Woolfardisworthy, Devonshire	Aug. 30	Aug. 25-27	S.W.	Kilhampton sub-district	7
21. Llandewi'rcwm, Breconshire	Oct. 23	Oct. 16-19	N.W., S.W.	?	...
22. Ridgeway, Pembrokeshire	Sept. 18	Sept. 10	W.N.W.	St. David's subdistrict?	25
23. Jamestown, Pembrokeshire	Nov. 17	Nov. 12, 13	W.N.W.	St. David's subdistrict?	25
24. Llanllyfni, Carnarvonshire	Sept. 26	Sept. 22, 23	S.S.W.	?	...
25. Glanaber, Carnarvonshire	Oct. 31	Oct. 26-29	E.N.E.	Denbigh? ...	30
26. Tremadoc, Carnarvonshire	Dec. 1	Nov. 24-28	N.E.	Garddllygaidydydd	5
27. Raunds, Northamptonshire	Nov. 15 1880.	Nov. 11	S.W.	Wellingborough	7
28. Forest Row (1), Sussex	April 1	Mar. 25-27	E.N.E.	Tunbridge Wells	10
29. Forest Row (2), Sussex	May 26	May 17, 18	N.E.	Forest Row(1)	1
30. West Hoathly, Sussex	Aug. 15	Aug. 11-13	E., N.E., E.N.E.	Forest Row...	4

Fig. 4.—Wind-mileage for 13 days preceding 30 Diphtheria dates.

The direction of the wind is taken from the Meteorological Tables published on the last page of the Registrar-General's Weekly Returns of Births and Deaths in London.

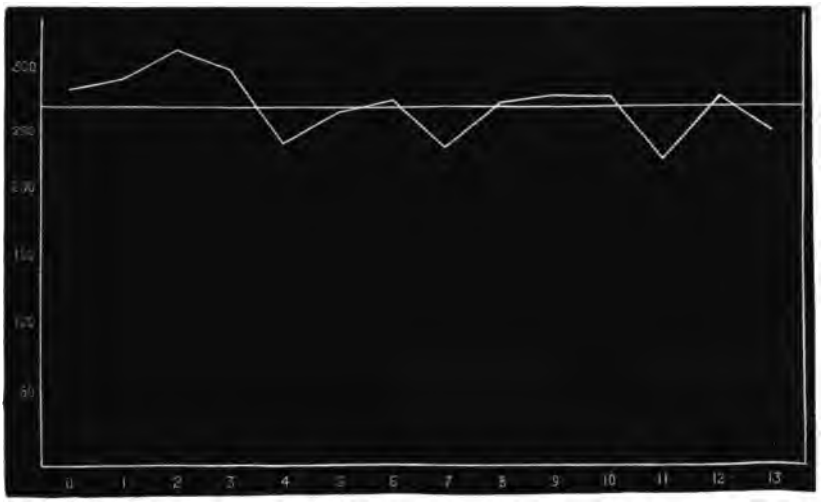

Fig. 5.—Wind-mileage for 13 days preceding 30 arbitrary dates.

Fig. 6.—Wind-mileage for 13 days preceding Diphtheria dates in 10 doubtful cases.

Those tables give the results of observations taken at the Royal Observatory, Greenwich. It would certainly be more satisfactory if I could produce, in each case, observations

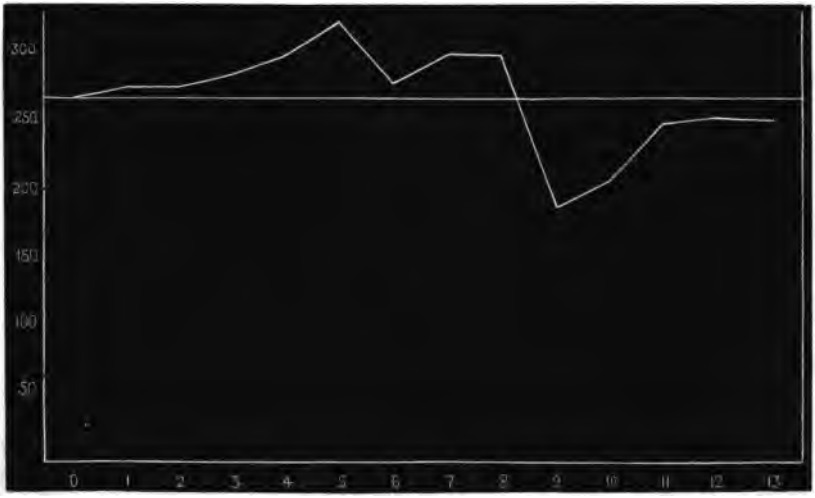

Fig. 7.—Wind-mileage for 13 days preceding *first ten* Diphtheria dates.

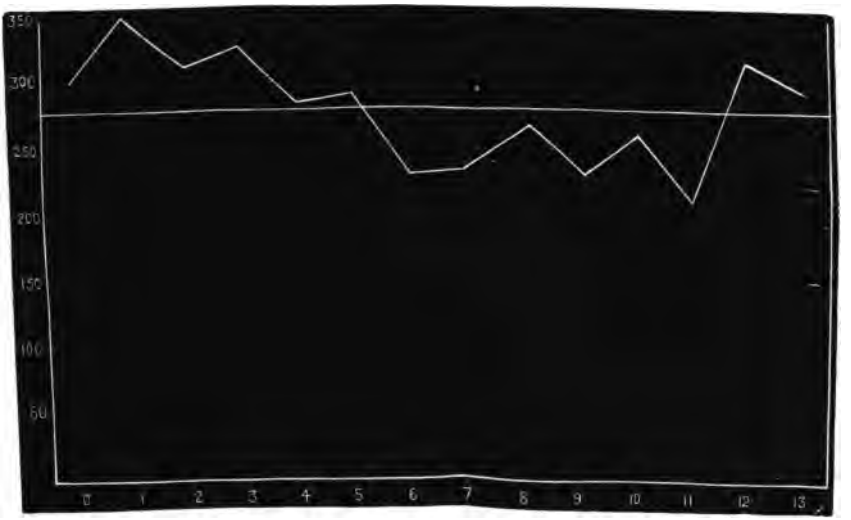

Fig. 8.—Wind-mileage for 13 days preceding *second ten* Diphtheria dates.

taken in the neighbourhood of the scene of the outbreak. This, however, would be a matter of very great difficulty, and is, perhaps, unnecessary for a first approximation ; though

Fig. 9.—Wind-mileage for 13 days preceding *third ten* Diphtheria dates.

in any future inquiry of the same kind it would be very desirable to secure it, if possible.

In each one of these thirty instances, the date of first attack was pretty accurately known, free from the ambiguity attending a general prevalence of sore throat; and the origin of the disease was quite obscure. The method of analysis employed was to extract from the Meteorological Tables for the fortnight preceding each outbreak, the daily direction of the wind, and the number of miles travelled in every twenty-four hours: then, to note in the reports themselves, or in the Registrar-General's Quarterly Returns, the occurrence of recent previous diphtheria in the surrounding districts, with preference for the nearest; and then to compare the direction of the wind (within probable limits of incubation of the disease) with the direction of any recent previous diphtheria.

By way of example, let me take the first instance on the list, viz. Coggeshall, a case which was thoroughly investigated by my friend and colleague, Dr. Thorne Thorne. The date of first attack, when two children were simultaneously and independently taken ill with diphtheria, was November 11th, 1875. The wind-facts for the previous fortnight were as follows:—

Day.	Direction of Wind.		Miles travelled by the Wind.
	A.M.	P.M.	
Oct. 29	E.N.E.	E. and E.S.E.	191
„ 30	E.S E.	E.S.E. and E.	149
„ 31	E.	E.N.E.	134
Nov. 1	E.N.E. and E.S.E.	E.S.E.	186
„ 2	S.E.	S.E. and S.S.E.	225
„ 3	S.S.E. and S.E.	S.S.E.	250
„ 4	S.W. and S.S.W.	S.W. and W.S.W.	213
„ 5	W.S.W. and S.W.	S.W.	291
„ 6	S.S.W., S.W., & W.S.W.	W.S.W. and W.	585
„ 7	W.S.W.	S.W. and E.N.E.	396
„ 8	N.E., N., N.W., and W.N.W.	W.N.W. and S.W.	394
„ 9	S.W., S., S.E., and N.E	S E., S.S.E., & S.S.W.	221
„ 10	S.E., S.W., and W.N.W.	W., W.S.W., & E.N.E.	392
First attack „ 11	N.E., N., and N.W.	W.N.W., W., and S.W.	400

Now let us turn to the Registrar-General's Quarterly Returns for 1875, and see what deaths had been registered from diphtheria in the surrounding districts. Coggeshall, I

may remark, is a large village in the northern part of what was formerly the Witham Union, in Essex, and is equidistant, about six miles, from Halstead to the N.N.W., Braintree to the W., and Witham to the S.S.W. Colchester lies about ten miles to the east. Let us examine the death-returns for the surrounding districts of Halstead, Witham, Braintree, Maldon, Lexden and Colchester.

		1875.				1876.
		First Quarter.	Second Quarter.	Third Quarter.	Fourth Quarter.	First Quarter.
Halstead	Halstead ...	1	1	...	...	...
	Hedingham ...	...	...	...	...	...
	Kelvedon ...	...	...	..	...	1
Witham	Witham ...	1	...	...	...	...
	Coggeshal ...	...	...	...	...	8
	Finchingfield ...	...	...	...	...	...
Braintree	Bocking ...	...	...	...	...	...
	Braintree ...	...	...	1	7	2
	Southminster ...	...	...	...	...	...
	Bradwell ...	...	...	...	...	...
Maldon	All Saints ...	...	...	...	...	1
	St. Peter ...	...	...	...	...	...
	Tollesbury ...	...	...	...	...	...
	Wivenhoe ...	1	...	...	...	...
	Peldon	3	4	...	...	...
Lexden	Stanway ...	1	...	...	...	...
	Fordham ...	1	1	...	...	...
	Dedham ...	...	...	...	...	...
	First Ward ...	2	1	1	1	1
Colchester	Second Ward ...	...	...	...	...	...
	Third Ward ...	...	...	...	...	...

In the first half of the year 1875, there had been a good deal of fatal diphtheria in the Lexden Union beyond Colchester, and a case or two in the Halstead district; but in the third quarter there appears to be no fatal diphtheria except a case at Colchester, and a case in the Braintree sub-district. In the fourth quarter, the same in which the disease appeared at Coggeshall, there were registered seven deaths from diphtheria in the Braintree sub-district. A foot-note by the district registrar states that, of the seven deaths from diphtheria, five occurred in Braintree and two in Black Notley, a village about two miles south of Braintree. It is evident that there was a serious outbreak of the disease in this quarter; and it is clearly in this direction that we must look for the parentage of the Coggeshall epidemic, if there is anything at all in the theory of aërial infection. What do we find? On the morning of November 6th (five

days before the outbreak at Coggeshall), the wind veered from S.S.W. to S.W. and W.S.W., and in the afternoon from W.S.W. to W., and on the morning of the 7th it was still W.S.W. Braintree lies due west, and Black Notley W.S.W. from Coggeshall. Moreover, the wind on November 6th travelled 585 miles in the twenty-four hours,—a rate of more than twenty-four miles an hour. In a quarter of an hour, the wind might have wafted infective matter from Braintree or Black Notley to Coggeshall. Can we suppose that so brief an exposure to the outer air would destroy the morbific vitality of the infective germ? Accepting the theory of aërial infection, how easy it becomes to explain the simultaneous seizure of two children in Coggeshall, who lived half a mile apart, and had no communication with one another.

Before I leave this case, I would ask your attention to the topographical features, which in some respects resemble those above described in speaking of West Hoathly in relation to Forest Row. The town of Braintree stands on high ground above the valley of the Blackwater. A west wind, blowing from Braintree, would flow in the trough of the valley as far as Coggeshall, but at this point the valley turns to the south, and the wind would press up the slope on which the chief part of the village stands. Thus Coggeshall, though by no means on a hill top, is decidedly exposed to the force of a west wind.

Several other cases in the list, *e.g.* No. 5 (Penkridge in relation to Stafford), No. 14 (Bleadon in relation to Wedmore), No. 15 (Staple Cross in relation to Rotherfield), No. 19 (Henley in relation to Needham), No. 27 (Raunds in relation to Wellingborough), No. 28 (Forest Row in relation to Tunbridge Wells), and No. 30 (West Hoathly in relation to Forest Row), present somewhat similar topographical conditions.

Each one of the thirty outbreaks in the catalogue requires to be minutely scrutinised in this way, but I must here content myself with stating the general results of the examination. "Of thirty outbreaks thus examined, twenty seem to satisfy the requirements of the wind-theory, that is to say, within eight or nine days of the first attack the wind was blowing [freshly] from a quarter in which diphtheria had recently prevailed at an average distance of seven miles; in nine [Nos. 4, 8, 17, 18, 21, 22, 23, 24, 25] the agreement is doubtful as regards the probable source of infection, though the wind-conditions [as regards velocity] appear favourable;

in only one [No. 13] are the wind-conditions apparently inadequate."*

I have alluded to the strength of the wind; and in examining these cases, one cannot help being struck by the frequency with which we find the date of an outbreak of diphtheria preceded, at an interval of a few days (averaging three to six), by a breeze of more than average daily velocity. I have heard it remarked by some medical men, who have had experience of diphtheria in rural districts, that these outbreaks usually occur after a strong east wind. The list of thirty outbreaks before us does not bear out this impression. Only thirteen of the thirty admit of reference to easterly winds, while seventeen followed winds more or less westerly. As a rule, I think it may be laid down that an east wind receives (practically, if not poetically) more attention than a west wind, and thus, perhaps, the impression above mentioned may be accounted for.

If we take the mean, for the whole thirty cases, of the number of miles travelled by the wind in each day of the fortnight preceding the date of outbreak, it distinctly appears that (on an average) during the five days immediately preceding that date the wind exceeded its average run, while during the previous eight days it was below the average. In illustration of this point, I must refer to Fig. 4 in my paper of last year, which shows the mean curve of previous "wind-mileage" for the thirty outbreaks in the list.

The representative character of this curve may be tested by dividing the thirty cases into three groups of ten each, and drawing the mean curve proper to each group. I have also picked out the ten cases which seemed (on other grounds) least likely to show agreement with the theory. Thus we have four partial groups to compare with the whole. The corresponding curves are shown in Figs. 6, 7, 8, 9. Whatever be their minor differences, they all have this large feature in common, that the week immediately preceding the date of outbreak was (on an average) more windy than the week previous; and so far they go to show that the main curve is not a compromise between discordant elements, but the fair representative of a number of constituents that do not greatly differ among themselves.

I may say that I have submitted this question to the judgment of friends possessed of the highest mathematical attainments, and have had their opinion that it was very

* "Observations on the Spread of Diphtheria."—*Transactions of the International Medical Congress*, 1881, vol. iv, p. 69.

improbable that such agreement could be merely the result of chance.

"We must not expect (even assuming the theory to be true) to find every case exhibiting close agreement with the wind-curve here presented, for, not to mention the probably wide limits of diphtheria incubation, there is the chance of error as to the date of first attack; the probability that some of the outbreaks were really due to unnoticed personal or mediate communication; and the possibility that, besides all modes of contagion, the disease may, at certain seasons of the year, arise through the agency of an external organism."* Indeed, bearing in mind all these possible sources of error, it is not a little remarkable that the evidence furnished by the wind-facts should be as strong as it is.

We may further test the character of the wind-curve by comparing it with a curve obtained in the same way from an equal number of arbitrary dates. Fig. 5 shows the curve obtained by taking the last day of each month in the years 1878 and 1880, and the 15th of September, October, and November. Fig. 10, in like manner, shows the curve due to

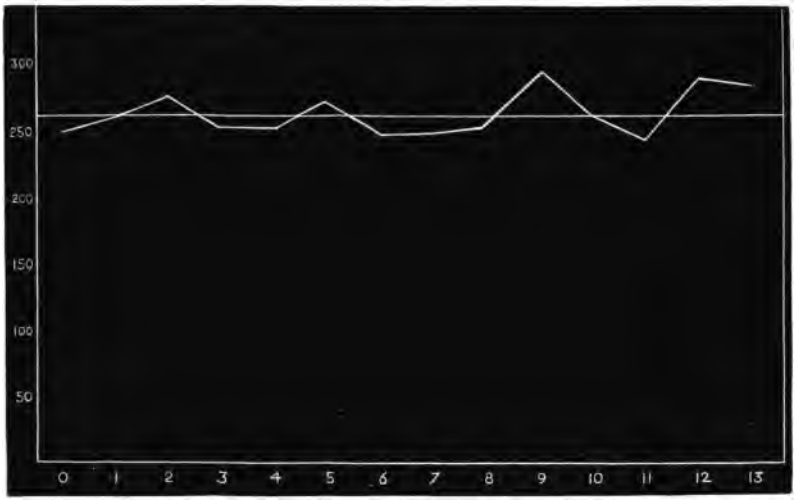

Fig. 10.—Wind-mileage for 13 days preceding other 30 arbitrary dates.

another set of thirty arbitrary dates, viz., the 21st of each month in 1879 and 1880, and the 7th of September, October, and November in each of those years. (I give the autumn months a larger share, to correspond with the greater fre-

* *Op. cit.*, p. 81.

quency of diphtheria outbreaks at that season.) In these two "comparison curves" we see only a capricious and random irregularity, whereas, in the curve belonging to the real diphtheria dates, I think we discern a large-featured structure, which appears to indicate a relation of some kind between the velocity of the wind and the occurrence of outbreaks of diphtheria.

If we had no other evidence to go upon, we might interpret this indication (supposing the force of it to be allowed) in various ways, according to the view we might hold as to the nature and origin of diphtheria. One theorist might contend that violent winds were the frequent cause of catarrh and sore throat, and that diphtheria was an infective development of simple sore throat occasioned in this way. Another might suggest that in windy weather there would be greater likelihood of sewer-air being expelled from sewers and inhaled by human beings, and so causing diphtheria. Perhaps other possible explanations might be suggested. But here comes in the significant fact that, in a large majority of these thirty cases, even with the imperfect information at our command, we can point to an actual focus of recent previous diphtheria, at no immense distance, from which the infection might have been brought by a wind which was actually blowing in the right direction at the right time. If this fact be established, and if its significance can stand a sufficient test, we shall be in a position to assert the probability that the relation which I have shown reason for suspecting to exist between the velocity of the wind and the occurrence of diphtheria outbreaks, is a relation of transport by the wind of infective germs from previously infected places.

The simplest test, and one which tries all the factors of the expression at once, is the same that we have already applied to the factor of wind-mileage alone, viz., to take at random a new set of thirty arbitrary dates, with such new wind-conditions as those new dates may entail, and apply them respectively to the thirty actual local conditions of outbreak. For dates I may take in succession the 21st of each month in 1879 and 1880, and the 7th of September, October, and November in each of those years.

"The result is that, in only ten instances out of the thirty, all necessary conditions appear to be fulfilled [that is to say, within ten or twelve days of the supposed date of outbreak there was a wind of more than average velocity blowing from a quarter in which previous diphtheria was known to exist]. In five instances the probable source of infection is doubtful; in thirteen, no probable source can be named; and, in the

remaining two, the wind fails to reach the required standard of velocity. Thus, the thirty real diphtheria dates satisfy the theory twice as often as the arbitrary dates. But, if the theory were groundless, the results ought to be very much the same with either set of dates. Thus far, then, we find the theory distinctly confirmed by the tests to which we have submitted it."*

These tests will require to be repeated, perhaps varied; but the chief desideratum is a much larger collection of authentic dates and localities of diphtheria outbreaks, subject to the limitations above suggested. If, instead of only thirty, I could produce 300 such dates, there would be little room for doubt about the character of the curve of wind-mileage. I am continuing to gather the necessary data, and hope, in course of time, to be able to lay additional evidence before this Society.

Now, there are one or two other points to be considered. In the first part of this paper, in describing two outbreaks at Woolfardisworthy and Forest Row, I drew attention to the elevated and exposed character of the situations in which they occurred. This is a point of considerable interest in relation to the question of aërial infection. I have already† brought forward evidence to show the comparative frequency with which diphtheria appears to select exposed places as the scene of its outbreak. Several observers have been struck with the fact; and it is important to ask what the meaning of the fact can be. It cannot be supposed that a bleak ridge is the favourite abode of miasmatic exhalations, or exceptionally liable to the influence of bad drainage and accumulated filth. If it were possible to imagine that mere exposure to wind and weather could cause diphtheria, we might certainly find these conditions in unusual force on elevated spots; but such an assumption would imply a denial of all analogy between diphtheria and other infectious diseases, and would demand the creation of a new school of epidemiology, for which we are at present unprepared. It is difficult to find any possible meaning in the preference of sporadic diphtheria for high bleak situations, except on the supposition that the disease may be caused by something that is wafted thither by the wind. The opposition that a hill offers to the great stream of the wind causes an increase of velocity in the wind as it rushes over the ridge; just as a river runs more rapidly where it is less deep. Any matters suspended in the air, which in a sheltered valley might possibly fall to earth, would be far more likely to remain sus-

* *Op. cit.*, p. 83. † *Op. cit.*, pp. 71-3.

pended as the wind swept them over the higher ground. But in all probability we have to deal with matters which would not fall to earth even in the stillest air,—possibly might tend to rise, or at least remain suspended in the same stratum of air. It would not be easy, in any given case, to forecast the probable path of any particle suspended in the air, especially where that path lay over a surface of land varied with hill and dale; but yet, in certain simple cases, we might do so in general terms. If, for example, an infective particle were launched upon the wind at the summit of a ridge,—the wind blowing across a deep hollow to a hamlet on an opposite ridge,—the particle, if set free in such a way as to escape local eddies, would probably be carried almost in a straight line, perhaps dipping somewhat over the hollow, but riding clear above any dwellings sheltered there, to the hamlet on the opposite ridge. In virtue of the greater velocity with which the wind sweeps over a ridge, it comes to pass that the different strata of air, which in the deeper and stiller parts of the air-stream are of considerable vertical depth, are all, in hurrying over the ridge, crowded and crammed together into a much shallower space, so that breathers on the ridge would run the risk of inhaling matters suspended in the upper and middle as well as the lower parts of the air. It may hence be apprehended, if there be anything in this wind-theory, that exposed places are the most likely to give, as well as to receive, infection.

Even supposing that the wind-theory should be found to hold good for a large class of derivative cases, I am inclined to think that a large residue will still remain that cannot be so explained. These, I expect, will be found to be what I may call "original" cases, occurring chiefly in certain districts noted for endemic diphtheria, and chiefly in the months of September and October; and I look hopefully to the day when that which originates the disease—the *bacillus diphtheriæ* itself—shall be detected in its native haunts, like the *bacillus malariæ* in the ague-stricken plains of the Maremma.

The great preponderance in number of diphtheria outbreaks in the month of October* appears to me highly significant of a dependence upon the seasonal development or reproductive activity of some organism having a life-history external to the human body; though it is, perhaps, not inconceivable that, even if the supposed organism passed no stage of its existence outside the human body, it yet might observe a certain season of the year in its development and reproduction. Germs derived from some previous case of

* *Op. cit.*, pp. 73, 74, fig. 1.

diphtheria might then conceivably, in virtue of seasonal habit, remain inert through the summer, and fail to germinate even though lifted by the wind and inhaled by a human being; while in early autumn their activity might

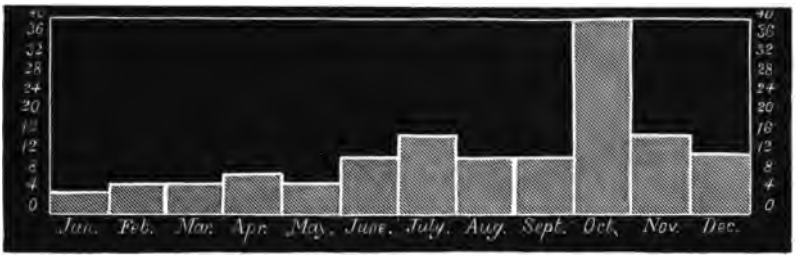

Fig. 1.—Monthly frequency of Diphtheria Outbreaks.

revive in virtue of season and ancestral habit, and cause an unaccountable excess of October outbreaks simulating spontaneous origination. Possibly a reference to the precedent wind-conditions might help in deciding between these alternative conjectures.

Thus far we have sought evidence only in the facts concerning diphtheria itself. If, now, we look for analogous phenomena in other zymotic diseases, our attention will surely be arrested by one particular disease, which, though not commonly classed among infections, certainly partakes of the zymotic character, and, in some important respects, admits of comparison with diphtheria—I mean ague. In its preference for certain localities, and for a certain season of the year, in its conveyableness by water, and in its origination independently of any previous case, ague appears to resemble diphtheria; while the contagious property of the latter is perhaps approached by that of yellow fever, which is closely allied to, and probably is only an intense form of malaria. Now ague is certainly conveyable by the wind. This point is well established by the facts adduced by Sir Thomas Watson in his *Principles and Practice of Physic*, vol. i, pp. 764-7. "In Italy," he says, "according to Dr. MacCulloch, the poisonous exhalations of the lake Agnano have been ascertained to reach as far as the Convent of Camaldoli, situate on a high hill, three miles distant." He alludes, also, to a "striking anecdote given by Lancisi, showing, on a small scale, the effect of the wind in carrying the malaria with it. Thirty ladies and gentlemen had sailed to the mouth of the Tiber on an excursion of pleasure. Suddenly the breeze shifted to the south, and began to blow over a marshy tract

of land situated to windward of them. Twenty-nine of the thirty were immediately after attacked with tertian ague." Hertz, writing on "Malarial Diseases" (in von Ziemssen's *Cyclopædia*, English translation, vol. ii, p. 572), gives evidence to the same effect, and sums up in these words: "We may admit the agency of the wind as a carrier of miasm for a short distance; but, when it comes to stretches of many miles, it is no longer to be taken into account." And if malaria can be conveyed by the wind, why may not diphtheria be conveyed in the same way? It would only afford one more point of resemblance between the two diseases. There even appears to be something analogous to the preference of sporadic diphtheria for high situations in the manner in which malaria, though mostly haunting low marshes, is sometimes carried up to hill-top dwellings by the wind.

I alluded at the beginning of my paper to the expectation we entertain of important evidence with regard to diffusion of small-pox by the air. I have myself met with an instance at Bourne Bridge, near Romford, in Essex, in which there was reason to believe that the variolous infection was conveyed by the wind a distance of two hundred yards, from a cottage where some infected bedding and clothing was being burnt, after a death from small-pox, to another cottage somewhat higher on the slope of a hill. At the latter cottage, a woman was taken ill with small-pox on the morning of March 27, 1881. She had had no communication whatever with the infected family at the foot of the hill, but she and her husband well remembered that, on the day when the infected bedding was burnt, the wind lay in such a direction as to bring the offensive smoke and smell of the burning right over them, causing them to leave their garden and take refuge in the house. This took place about six o'clock in the evening of March 14, which would allow an incubation-period of twelve days and a half, supposing the woman to have received the infection on that occasion.

It might be expected that some positive evidence would be forthcoming as to the carrying of influenza by the wind; but, beyond general surmise, there seems to be little solid information on the subject. Zülzer (*Von Ziemssen*, vol. ii, p. 526) refers to Biermer as arguing "that the apparent contradictions of rapid diffusion and slow migration, of general extension and local limitation, in the history of influenza, would be easy of explanation on the theory of a living miasm, which would be capable of being carried onward by the air, but had an independent existence of its own, and would find in certain places conditions more favourable for

its development than in others." This view, I may remark, corresponds closely with that to which we are led by many of the facts concerning diphtheria. But Zülzer adds, "an established basis of facts is meanwhile wanting for this hypothesis, as well as for all the others."

In passing, I may mention hay fever as an ailment which, though not contagious, is yet a certain example of illness caused by the inhalation of particulate matter carried by the wind.

The objection most readily urged to the theory of aërial infection is that exposure of infection germs to air speedily destroys them, probably by a process of oxidation;* but I would ask whether this assertion rests upon any sound base of scientific observation and experiment. Curschmann's dictum with regard to small-pox, that "exposed to the atmospheric air, the contagion is readily destroyed, most probably by being more and more diluted in the air, until it becomes inefficacious, and finally disappears",† will hardly be accepted at the present day in face of evidence to the contrary.

Even if proved for one kind of infection, such a frailty of vital endurance would not necessarily attach to another kind of infection. Let the evidence for each member of the class of infectious diseases be considered on its own merits, and not be set aside on the strength of a hasty generalisation.

The theory of aërial infection is not in the least opposed to established ideas concerning the spread of zymotic diseases. It only supplements established ideas. It offers the means of explaining facts and phenomena which at present baffle all inquiry, and which seem likely to continue to do so until some additional ideas are admitted to a share in the task.

Abundant admission of fresh air into a fever-room may very likely be beneficial—to the yet healthy inmates of the house, by expelling the already infected air,—provided the infected air be not thereby carried to some other part of the dwelling; but it would be an unfounded assumption to suppose that the outletting of infected air could not possibly be dangerous to the passers-by in the street outside.

As to the changes and chances of the mortal life of bacteria committed to the air, we have, I imagine, little but analogy to guide us to an opinion. Professor Tyndall found that exposure to a certain heat was sufficient to kill adult and growing bacteria, but left the germs still living, and that it was not until the younger generation had become adoles-

* *Trans. Internat. Med. Congress*, 1881, vol. iv, p. 90.
† Von Ziemssen, *Cyclopædia*, vol. ii, p. 336.

cent that they could be destroyed by that degree of heat. May there not be something similar in the action of air on the infection-organisms,—destroying some and leaving others alive? But we do not even know in what form, or in what stage of development these organisms are given off by the sick. How then can we venture to dogmatize on their fate when given to the air? It is reasonable to think that the element of time must be an important factor in the result of exposure. The theory which I have been considering only demands for the morbific particles an average exposure of about half an hour. Those, at least, who are inclined to suspect for the diphtheria organism the possibility of an independent existence in haunts external to the human frame, will not be disposed to regard half an hour's or an hour's exposure to the air as likely to prove more fatal to this organism than it is to the *bacillus malariæ*.

To my mind, it is not the risk of destruction but the certainty of dilution of the poison, by the air, that seems to constitute the chief *à priori* difficulty in the way of the aërial theory of infection. One is apt to suppose that the morbific particles must become so widely separated by the whirlings and eddyings of the air that little chance can remain of any sufficient number of them being inhaled by the same person to give rise to the disease. But even here we are raising an objection upon the basis of an assumption. How do we know that one single germ inhaled by a susceptible person may not be sufficient to cause the disease? The chance of such inhalation must necessarily diminish with the distance from the centre of dissemination, though the rate of diminution may be subject to great variation. Certain conditions may cause the arrest of the germs, as the poison of malaria is believed to be arrested by trees and by water. Other conditions may cause them to be upborne into the higher strata of the air, and carried over the country clear above all habitations. Under other circumstances an invisible cloud of deadly dust many hang together in the air and be drifted by the wind to the very door of the farmhouse on the hill.

THE POLICY AND PRACTICE OF GLASGOW IN THE MANAGEMENT OF EPIDEMIC DISEASES, WITH RESULTS.

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DIFFERENTIATION of function is the characteristic feature of the growth and development of communities as it is of the evolution of animal life into higher forms. In the course of time, the various organisations essential to the existence of a community shape themselves out in response to the increasingly complex conditions of its life; but it is only after many trials and many failures that the various functions necessary to be performed are properly distributed and discharged. The care of the public health in a city is one of the most important of these functions. It concerns every class, in all circumstances. Yet it will be found, especially in old countries, where municipal development has followed internal local impulses through many centuries, that the proper differentiation of this function is more slowly attained than that of any other. The reason seems to be this, that there is something in public health, and the measures required for its conservation, more or less akin to the objects of several of the departmental organisations of a community which come into existence prior to the recognition of the public health as an imperious subject of administration. The tendency, therefore, naturally is to try each of these organisations in succession, as the organ of this new function, or to distribute the function among them, deriving a little service from each as occasion presents, or as their primary ends permit, in the hope that thus the entire area of public health requirements may be covered.

There are thus recognisable in the history of cities distinct stages in the evolution of a policy in public health administration. My object now is to illustrate these stages from the history of Glasgow. It is only with that department of public health administration which relates to the management of epidemic diseases that I propose to deal. But it is notorious that the fiery heart of all the energy ever thrown

by cities or nations into sanitary work is the epidemic. At first, this energy is only a spurt put on to pull the boat past threatening annihilation; but, gradually, the steady, quiet swing of daily work makes those spasms of excited exertion unnecessary. Not only is epidemic disease always and in all places at the centre of sanitary activities of all kinds, but of all the objects of sanitary administration, the control and repression of epidemic disease depends most for a successful issue upon proper differentiation of function. But there must be no conflict of interests, no diversity of dealing with classes in the community, no division of the community into separate jurisdictions, or distribution of sanitary functions through independent departments, otherwise, failure is the inevitable result. As regards the detail of practice in the management of epidemics—isolation, disinfection, the special items of practice adapted to combat the special conditions which promote individual epidemics—there is a tolerably uniform level of attainment of knowledge everywhere. Nay, more. On these matters, it is startling to note how, in the main, the great movements and operations in the battle against infectious disease have been understood and intermittently practised in times of supreme danger, for generations, if not centuries. In the practice we have not much to learn, but in the policy a great deal—in the concentration of the function, in the guidance of the practice, so that there may be uniform and united action over the entire area of what ought to be in reality, as in name, the community. All the weapons of the sanitary armament ought to be stored in one armoury, and wielded under one command.

The first glimpses of the dealings of the authorities of Glasgow with epidemics are obtained in the sixteenth century. The population then averaged 5,000, and the magistrates and Town Council administered the comparatively simple affairs of this small community with but little official assistance. From 1350 they had been provided, by private beneficence, with a Leper Hospital, to which patients were regularly sent by the magistrates; but the plague, which invaded the inhabitants at frequent intervals from a very early date, first tested their capacity for coping with epidemics. In anticipation of its appearance in 1574, the Burgh Court drew up a series of ordinances, which were subsequently revised from time to time, and which were marked by considerable intelligence. Their main substance is a rigid system of quarantine, extending both to persons and goods. Wardens of the ports of the city were appointed for that purpose. Other items in the ordinance were more akin

to our modern practice. No lodgers were to be received by any one who had not a "licence" to do so from the magistrates. The duty of immediately reporting cases of sickness was imposed upon all householders, under pain of banishment—a source of information which the sanitary department of Glasgow at the present day does not enjoy. "Searchers" were appointed to specified districts, whose business it was to go round their district morning and evening to discover any who might be seized with the disease—an anticipation of the house-to-house visitation of the present day, which I have heard claimed as a practice originated during the first invasions of cholera at the beginning of this century. There was, even at this early date, a foreshadowing of the sweeping operations of our Improvement Trust in the last item of the ordinance—"Ordains the School-house Wynd and all the Vennals to be simpliciter condemned and steekit up". Nothing is said as to medical aid for the infected, but probably this was not neglected, as, three years after, the Burgh records show that a surgeon was retained for the service of the town, at a yearly salary of ten merks. There are numerous subsequent references to this official, which show that the magistrates always had in their service, at an annual salary, or retained by special fee, a practitioner for attendance upon the sick poor. The ordinary wants of the poor were provided for by the Kirk Sessions; who derived their funds, first from church door collections, and, when these were insufficient, by levying an assessment (1595). The incorporated crafts and the merchants' guild also maintained their poor members, but, down to the eighteenth century, the only provision for medical attendance and medicine for the sick poor seems to have been made by the magistrates from the funds of the Burgh.

In 1646, when the population had almost trebled its numbers, there was another outbreak of the plague, against which more elaborate and advanced measures were adopted by the Town Council. Wooden huts were erected on the Town's Muir at a considerable distance from the town, to which the infected were transported, and where they received medical attendance, and were maintained at the charge of the Burgh funds. A superintendent was appointed, with special instructions to "take notice of the graves". A staff of men was engaged to clean infected houses; they were provided with horses and carts to remove the infected clothing to the muir, there to be "cleanged". A "close cart" was obtained to transport infected filth beyond the bounds; all these measures showing an advance in the understanding of the circum-

stances which promote the spread of infectious disease, beyond the mere quarantine regulations adopted in former visitations. Rich and poor seem, at the height of the epidemic, to have been isolated and assisted, free of charge, at the cost of the common funds, as, at the end of next year, the bailies determine to exact payment for the services of the "cleangers" from those householders who were in a position to pay. But the point which is of special interest to us at present is this—that all the measures deemed necessary for the suppression of the epidemic were adopted and carried out by the Magistrates and Town Council as representing the community. Councillors were appointed in rotation, whose business it was to visit and inspect the huts on the muir twice or thrice a week, and every Saturday to give in a list of the inmates and of those who had died. Of course, this unity of policy and practice was natural in those simple times, when those fathers of the people personally cared for the infected just as they personally exercised the functions of police, and otherwise governed the citizens very much as a father exercises rule over his own household. Still we have here a principle which we shall see was abandoned in the more complicated circumstances of a larger population, provided with other organisations which were useful and necessary in their own sphere, but which divided the responsibility in reference to epidemic disease—a principle, moreover, which has only in recent times been recognised and again put in practice.

In 1818, Glasgow found itself in the middle of its first great epidemic of typhus fever. It was now a manufacturing city of about 145,000 inhabitants. Of this population, one-half lived beyond the Burgh boundaries, under separate jurisdictions, so that whatever might be the nature of the measures adopted to cope with the epidemic within the jurisdiction of the Burgh magistrates, the territorial unity of the community was destroyed. Beyond those limits they had no administrative authority, and thus one great element of success in dealing with epidemic disease, which existed in the case of the plague, was now impossible of attainment. In other respects the community was more favourably placed. The Royal Infirmary had been opened in 1774 and enlarged in 1816, so that there was now accessible a general hospital with two hundred and thirty beds. But this was the public hospital, not only of the Burgh and more immediate suburbs, but of villages and country parishes for many miles around. Liberal donations towards its erection had been received from the corporate bodies, parochial authorities, and private individuals over an enormous area. Annual subscriptions

were contributed from the same sources, so that the managers of the hospital found themselves besieged by fever-patients from all quarters. Within the city, and provided exclusively for the use of the poor of the city, a building called the Town's Hospital had been erected in 1773 at the joint cost of the magistrates, the kirk sessions, the trades' house, and the merchants' house, the bodies upon whom still rested the care of the poor. An assessment was levied upon property by a joint board representing these bodies. This institution was not, however, for the treatment of the sick, but for the reception of permanent poor and the education of orphaned or deserted children. The casual sick were attended at home by district surgeons or sent to the Royal Infirmary for treatment, at the cost of the corporate bodies mentioned. The immediate result of this arrangement was that the accommodation was, as the epidemic extended, more and more absorbed by cases of fever. From 1795 to 1814, it appears that 13 per cent. of all patients admitted suffered from fever. This proportion rose in the next three years to 40 per cent., and in 1818, at the height of the epidemic, it actually reached 60 per cent.; and this although the managers were compelled for a time to shut their doors. The same tragic drama was now enacted as was witnessed on the occasion of every subsequent epidemic up to quite recent times. A Fever Committee of citizens was appointed, public subscriptions were collected, and, while the epidemic raged and finally burned out, efforts were made to get sites for fever-hospitals, which were thwarted by the opposition of the neighbourhood. The final outcome of these efforts at this time was thirty-two additional beds, obtained by adapting a private residence—a result the practical advantages of which are correctly estimated by the saying of a physician of the day: "It is like extinguishing a conflagration with a single bucket."

It is in one sense humiliating, though in another a legitimate cause of pride, to read the medical pamphlet literature of the day, and recognise the intelligent completeness of the advice tendered at this crisis by the profession in Glasgow to the authorities. It is no exaggeration to say that in these pamphlets may be found a perfect exposition both of the policy and practice which alone can be successful in dealing with epidemics. I do not include in the scope of this paper the routine work of sanitation which would prevent epidemics—enforcement of cleanliness within and without the house, suppression of overcrowding, regulation of lodging-houses, demolition of unhealthy tenements, opening up of densely-populated districts; but all that was pressed upon

the attention of the public, and nothing of that was done, until it became apparent, fifty years later, that existence was impossible without such measures. As to the procedures immediately bearing upon the suppression of a present epidemic, the advice given was quite as pointed and perfect. Erect fever-hospitals, in anticipation of future necessity, organise a staff of fumigators or disinfectors; lime-wash the houses; disinfect the clothing; burn the infected and supply new bedding; provide ambulances instead of allowing the public sedan-chairs to be employed to transport fever-patients to the infirmary; fix the proper number of inmates for the smaller houses, and employ the police to enforce the regulations. Yet even in respect of such a simple measure as disinfection of the houses from which cases of fever had been removed to the infirmary, we read, six years afterwards, in the annual report of that institution: "Measures have at the same time been taken to have the houses the patients left cleansed and fumigated. The directors have thus, they hope, done all in their power for staying the contagion." That is to say, the so-called public authorities did nothing but send into this charitable institution the fever which was bred in physical conditions which they ought to have removed, there to be treated at a loss to the funds; and not only so, but they permitted the managers still further to deplete these funds by disinfecting the miserable houses from which their patients were removed.

The injustice and inefficiency of this method of using the charitable organisation of the infirmary as an instrument for the suppression of epidemics, as a matter of public policy, was amply dwelt upon, both in the reports of the infirmary and in the medical literature of the day. It was unjust, because the magistrates of Glasgow were absorbing the accommodation and devouring the funds provided by the suburbs and rural districts. The infirmary was a sort of copartnery for charitable purposes; and yet a few of the partners were monopolising the whole benefit, or, at any rate, a greatly disproportionate share of it. It was also pointed out that the system of patronage necessarily attached to the method of maintenance by subscription, and the consequent limitation of access to the institution to the channels opened up by subscription, was fatal to the utility of the hospital as an agent in the prevention of fever by isolation. One of the physicians to the infirmary wrote: "There must be no patronage of individuals; there must be no roundabout application necessary to the magistrates, governors, clergymen, or elders." In short, the only passport must be a certificate of

fever from a medical practitioner. That must procure immediate entrance into any fever hospital which is to be of real service to the community. The same gentleman illustrated, with equal force, the absurdity of territorial limitation of the right of admission, when all came from one community. He wrote: "Is fever to be deterred by the barrier of a few Royalty stones, dropped in its passage, though each be regularly numbered, and each have the letter R, as if it were a talismanic character, engraved upon it?" This remark was drawn forth by the resolution of the Glasgow Fever Committee to exclude from their Special Fever Hospital all suburban cases, after having crammed the infirmary, and so cut off its resources also from the suburbs. Later on, I shall return to the question of policy raised by this proposal. Meanwhile, it is enough to point out that it is impossible to combine two methods of dealing with epidemics—the one by having recourse to the charity of a community, the other by assessment, whether statutory or voluntary, of a section of that community. To attempt this is both to be unjust and inefficient.

We have now obtained a fairly accurate idea of what continued to be the policy and practice of Glasgow in reference to epidemics for well nigh fifty years, the population, meanwhile, increasing *annually* from 2 to 4 per cent. in successive decades, with an influx of Irish, so that their proportion averaged 17 per cent. actually Irish-born. Throughout this period, through all the vicissitudes of trade and popular distress to which a great manufacturing population is exposed, through epidemics of typhus, cholera, relapsing fever, and small-pox, the Royal Infirmary of Glasgow continued to provide hospital accommodation for these diseases, and its managers to perform the unenviable task of extracting the funds necessary for this purpose from the harrassed public and the unwilling and stingy authorities, both municipal and parochial, by alternations of coaxing, reasoning, and threatening, as seemed best to suit the immediate circumstances. In 1829, they built a permanent Fever-house, which was enlarged in 1832, and then contained 220 beds. From time to time they erected temporary wooden booths, or "fever-sheds", on their own grounds, or took charge of temporary district hospitals in the city and suburbs, erected either by themselves or by the magistrates. They had even to oppose, at their own cost (and to appeal, happily with success, to the highest courts of law), an application for interdict, raised by the neighbourhood against the use of one of these hospitals. As may be anticipated, these spasmodic provisions were

never made until disease had outrun the accommodation, which was therefore never, at any time, adequate to meet the emergency. In the five years, 1827 to 1832, only one-fourth of the ascertained cases were treated in hospital. Dr. Cowan tells us that, from 1827 to 1840, there were 9,665 patients treated *at home* for fever by the district surgeons. Dr. Perry states that in 1843 there were 12,937 cases of relapsing fever attended by the district surgeons, of which only 4½ per cent. were sent to the Infirmary; and still, with a philosophy worthy of Mark Tapley, these gentlemen extract this consolation out of this suicidal policy, that, but for the *liberality* of the Directors of the Town's Hospital in allowing a distribution of money to the sick at home, at least one-third of the 12,397 cases treated at home would have been necessarily sent into the Royal Infirmary at 15*s.* each, costing £3,074 instead of £284, the amount which they distributed! This miserable subsidy of 15*s.* a piece for the treatment of a case of fever is a fair illustration of the huxtering policy which the Directors of the Infirmary had to encounter in their numerous attempts to extract from the public authorities a charge for the maintenance of the patients with whom they burdened the institution. Year after year they were compelled to urge, under the pressure of their dire necessities, those inconsistent and mutually destructive arguments. To the managers of the Local Poor-Rate and to the magistrates of the Burgh they said:—"You are throwing upon our voluntary subscribers burdens which you ought to bear; you are saving your public funds by consuming our capital, which is derived from legacies and donations, the interest of which was intended to eke out our expenditure for the relief of general sickness." To the public they said:—"A contribution is fairly due from every one, as a *premium*, for the protection and security afforded" (1832). "Our Institution is not merely an Infirmary for the treatment of the diseased poor; it has become *an establishment of Medical Police—a Lazaretto for the seclusion of an infectious disease*" (1833). This inconsistency of argument is another proof of the injustice and inefficiency of endeavouring to treat infectious diseases from private charity instead of from public assessment.

These dismal facts in the experience of Glasgow and other large Scotch towns were not passing unobserved by men capable of extracting a healing medicine from their bitter fruit. Drs. Cowan and Perry in Glasgow, and Alison in Edinburgh, were such, and their names will always deserve honourable mention in the records of a philanthropic and

philosophic public policy. Three suggestions of measures of primary importance for the public health and general social well-being were derived from the condition of affairs which I have described, and which were urged with the greatest ability and perseverance, by every channel which could reach the intelligence of the public and the legislature, until they ultimately passed into definite enactments.

As Dr. Alison pointed out, "The Statute Law of Scotland *requires* the leaders, ministers and elders of parishes, and the magistrates of boroughs, to make provision for the needful sustentation of all aged poor and impotent persons, to enable them to live unbeggared, and to tax and stint the inhabitants, when necessary, for this purpose" (1840). We have seen that this was done in Glasgow. It was also done in Edinburgh and the chief towns of Scotland; but in the rural districts the law was either neglected, or, for selfish purposes, so administered that "the charities, legal and voluntary, of large towns were burdened by persons from other parts of the country." Dr. Cowan found that, of 178 inmates of the Royal Infirmary, in April 1840, only 38 were natives of Glasgow and 98 had not passed the prime of life there. Dr. Perry found that not more than 15 per cent. of the fever patients admitted under his care were natives, and 25 per cent. had not been three years resident; that 30 per cent. were natives of Ireland, and 40 per cent. from the Highlands and agricultural districts of Scotland. In 1840, Dr. Cowan states that "no effective measures have yet been taken to place the indigent poor of the suburbs [of Glasgow] under a system of medical superintendence, similar to that within the borough." In 1845, all this was remedied by the passing of the Scottish Poor-Law Act, which gave to Scotland the benefit of an organisation which England had enjoyed since the reign of Queen Elizabeth. This created, for the first time, a class in the community for whose sickness, whether from infectious or ordinary diseases, statutory provision was made and a statutory mission imposed upon a new social organisation to support this provision out of public assessments. As in the case of the charitable aid of the Royal Infirmary, this was, in its time, a great blessing to society; but it complicated the development of an adequate public policy in the unification of the management of infectious disease, which is incompatible with the acknowledgment of any class in society, whether pauper or beneficiary.

Another measure was one which had for its object the union, under one municipal government, of the community of Glasgow, which had by its extension become territorially

divided under separate and independent local administrations. We have seen that while there were district surgeons within the Burgh, there were none in the suburbs. In 1840, Dr. Cowan sums up the whole position thus:—"Over the city and suburbs there are four independent magistracies and boards of police, four assessments for the poor's rates, and four modes of administering the poor's funds, equally independent of one another." You will remember that in 1818, the impotence of boundary stones to control the movements of fevers in a continuously inhabited area was graphically pointed out. In 1841, the entire community numbered 279,000 inhabitants, of whom 37 per cent. were under three jurisdictions distinct from Glasgow proper. Yet, for Parliamentary purposes, this population had been made one by the Scotch Reform Act of 1832. The enormous social and sanitary disadvantages of this territorial subdivision and antagonism were forcibly and fully pointed out by Mr. Charles Baird in 1841, in a "Report on the Legal Provisions Available in Glasgow for the Removal of Nuisances", made to the English Poor-Law Commissioners, who embraced Scotland in the scope of a Parliamentary inquiry. All these facts resulted in the consolidation of the entire Parliamentary area under one municipal government by the amended Glasgow Police Act of 1846. It contained general sanitary provisions of an important kind, including the regulation of lodging-houses, and the reporting of infectious disease among their inmates, with permissive powers to cleanse and disinfect houses and clothes. It is right to say that these sanitary clauses existed in the Police Act of 1843, and were now only extended in their operation over the entire community. Thus, for the time, another element of confusion in the application of a uniform policy in the management of epidemic diseases was removed, viz., division of jurisdiction in the community; but only for a time. We shall shortly have to point out how, by the progressive growth of the city, the territorial unity was again destroyed, and to state how this difficulty has been circumvented, if not removed, in so far as it affected the control of infectious diseases.

The third, and, in its ultimate issue, the most important measure which was suggested by the difficulties of Glasgow and other towns in coping with epidemic disease was the vesting of responsibility for their entire management in public bodies, having powers of assessment for this purpose, and general jurisdiction over areas determined as far as possible by their natural adaptation to the attainment of the ends in view. This was effected locally by the Glasgow

Police Act of 1862, confirmed and amended in 1866, and generally for the whole country by the Scotch Public Health Act of 1867, both of which are to this day the local and general Acts in force in Glasgow and Scotland respectively.

It is necessary to hark back a little, in order to show in its proper historic relation the policy of the new Poor-Law organisation in reference to epidemics. Being sectional in the legal obligations which it bore to those attacked by epidemic disease, obviously it would afford only sectional aid in dealing with an epidemic. But parochial boards were empowered, by the Act, to subscribe from their funds to any public hospital or dispensary, and, therefore, they still looked to the Royal Infirmary for the accommodation of such of their sick as seemed to require hospital treatment. The effects of this policy were soon made apparent. In 1845, typhus was at the lowest ebb ever known in Glasgow. This was but a lull before the storm; and, in 1847, the city was overwhelmed with the most extensive epidemic in its history. In the previous year, more than half the cases admitted to the Infirmary were sent in by the parishes. At its close the accommodation was exhausted. Then followed the usual provision of temporary accommodation at the cost of the Infirmary, and, at last, when the rejected applicants for admission numbered hundreds, the parishes set vigorously to work, and, by the adaptation of the old Town's Hospital wholly for a fever-house, and the erection of wooden sheds, 1,024 additional beds were provided, raising the entire number in the city to 1,254. By these means, in 1847, no less than 11,425 cases of typhus received hospital accommodation. This was practically all that was done to stay the pestilence. Only when it was dying out was any disinfection of the houses or washing of the bedding attempted. Next year, the Infirmary managers report that the parishes had closed their hospitals, and prevailed upon them to reduce their charge for fever cases from £1 to 15*s.*, a sum, they state, "by no means adequate to cover the average expenditure on each case, but of which, co-operating as they were with the authorities for the public good, they still felt it their duty to accept" (1848).

With similar local assistance, the city encountered subsequent revivals of typhus and epidemics of cholera and small-pox, until, in 1862, the new Glasgow Police Act brought the municipal authorities upon the scene, or, rather, conferred upon them powers which were not exercised, so far as hospital accommodation is concerned, until 1864, when the managers of the Infirmary again found themselves unable to

cope with epidemic typhus. They held "numerous conferences" with the Parochial Boards and the Police Board, and wisely stated that "it appeared to them that it lay more properly within the province of the Police Board, under the powers conferred upon the Board by the Glasgow Police Act, to make provision, in times of epidemic, for the treatment of disease" (1864). Thus, by a gentle impulse, the municipal authorities were made to enter upon the task of providing hospitals for themselves. But here I must interrupt the current of my narrative to say that Dr. Gairdner was appointed the first Medical Officer of Health possessed by Glasgow early in 1863, and immediately began the herculean task of laying the foundation of the entire sanitary service of the city. As I have already said, it is only the treatment and general management of existing epidemic disease I am at present discussing, but I need scarcely say here that the roots of epidemics can be reached only by an efficient sanitary department, fully organised in all branches of its work. The hard labour, the slow education of public opinion, the reasoning down of opposition, the conciliation of angry interests—all this fell to the lot of Dr. Gairdner, and no one feels more grateful to him than the present writer, whose happy fortune it was to be preceded by such a man, and thus, in a measure, to reap the fruits of his labour. It must, therefore, be remembered in all I have now to say that, contemporary with the various movements in the development of the present policy in regard to epidemic disease, there was very slowly, but still steadily, coming into operation all the departmental work which is essentially necessary to the ultimate success of any policy whatever in that regard.

The 1862 Act was tentative in some of its provisions, especially with reference to dealings with epidemic disease, and showed that the mind of its framers was not clearly made up as to the policy to be pursued. Wisely, therefore, it was provided it should remain in force only for five years. As it happened, during those five years the Municipal Authorities learned many things. Having been taught all that was known of sanitary practice, its method, and the occasion for their use, under the lash of epidemic disease, it is not surprising that the legislature should have in such a school learned a system of spasmodic sanitation. Such was the method of applying Privy Council orders—legal instruments, under the authority of which affrighted authorities proceeded to administer, in drastic doses, that which they ought to have, from day to day, exhibited as a mild tonic to keep the body of the community in constant health. This evil method

was introduced into Scotland in the Nuisance Removal Acts, various editions of which were passed from 1846 to 1856. They conferred no effective powers for ordinary times, no medical officer, and only in extremity of epidemic pressure, by Orders in Council, duly published in the *Gazette*, and for specified devoted localities, brought into operation house-to-house visitation, suppression of overcrowding, special cleansing, power to dispense medicine, and provide medical attendance and hospital accommodation. As was natural, and, indeed, unavoidable, in their first Local Act, the Glasgow authorities followed the general Act, merely instituting a minor grade of treatment by proclamation of "districts, streets, or courts" within the city, advertised in the local newspapers, holding in reserve the Privy Council and the *Gazette* for the direst extremities. These special powers were invoked upon the report of the medical officer "that epidemic, endemic, or contagious disease prevails or exists and threatens to prevail." He was, therefore, placed in the position of a fire-master, who had instructions to watch the progress of a fire until it had assumed, or threatened to assume, the dimensions of a conflagration, at which supreme moment he was required to shout for the fire-engines. Then the fire-engines had to be made, for it was carefully pointed out by the legal advisers of the authorities that they had no power to expend their moneys upon permanent structures. The clauses in the Police Act had strict reference to emergency; and, in fact, their powers must be renewed every six months, with due form of advertisement or proclamation in the *Gazette*. Dr. Gairdner had, in the first place, to choose between the risk of crying "fire", and incurring the ridicule of the public who turned out to see a good blaze and could perceive nothing but smoke, not to speak of the anger of the proprietor, whose property had been unnecessarily made notorious, and the recriminations of the same public if, by misjudged delay, half the city had been wrapt in flames. When the supreme moment did arrive, which was in the winter of 1864-5, he was told that "building of any kind was in that season possible only under great disadvantages, and building of stone or brick absolutely impossible." He, therefore, looked around for a building which might be quickly adapted; and, after many rebuffs, obtained the offer of a large disused mill. No sooner did this project get wind, than a perfect storm of indignant remonstrance gathered in the neighbourhood, burst upon the Medical Officer, and swept his temporary fever hospital at once into the Limbo of vanities. There was nothing for it but to build, and, as

everyone was now convinced of the necessity of making hospitals a permanent possession of the community. Lawyers notwithstanding, a site in the northern quarter of the city was purchased, and, in less than three months, a substantial pavilion hospital, partly brick, partly wood, was erected, furnished, and opened, with one hundred and thirty-six beds, on 25th April 1865. Premises had been obtained elsewhere for washing and disinfecting clothing in 1864; and a staff for fumigating and lime-washing houses had been organised. While these efforts to acquire hospital accommodation of their own were being made, the municipal authorities had provided for cases in the parochial and infirmary fever wards; but they at once discovered that there were many persons attacked who ought, for public safety, to be isolated, but who were not paupers, whom the parishes would not receive, and whom, in fact, it would have been little less than a social outrage to force into the pauper ranks. These were sent to the Infirmary, but could not always be received. Even among those who were brought by fever within the legal definition of a pauper, I found, while in charge of the largest of the parochial hospitals, that only five per cent. had ever before been within the walls of a poor-house. Here, therefore, was a system of pauper manufacture which, for social as well as sanitary reasons, it was of the utmost importance to terminate. When, therefore, the 1862 Act fell to be revised in 1866, the Police Board bound the community to maintain their hospital and washing-house, and took powers to enlarge the same or erect others, as occasion required. The spasmodic system of sanitation was so modified as to permit of the routine use of every known method of preserving health and controlling epidemics in their rudiments. In reference to infectious disease in lodging-houses, the power of compelling removal to the municipal hospital was substituted for that of compelling the Poor-Law Inspector to give medical attendance. The general Public Health (Scotland) Act, passed next year, enlarged the power of compulsory removal so as to compass all cases of persons living in circumstances of special individual necessity, or of special public danger, and otherwise; between the Local and the General Acts, the municipal authorities found themselves vested with power to do anything and everything for the prevention and control of infectious disease under their own hand, out of the assessment levied upon the community for sanitary purposes—an assessment which, in the seventeen years during which it has been imposed, has been for eleven years one penny in the pound of rental of houses valued below £10, and twopence

in the pound of houses valued at £10 and upwards; for three years, half these rates; for one year, double; for one year a half, and for one a fourth more than those rates.

It may be well to lay before you, in mere outline, the sanitary organisation and equipment as it now exists in Glasgow—a city of 514,000 inhabitants, living on an area of fully nine and a half square miles.

1. A Medical Officer of Health, a Chief Sanitary Inspector, and a Vaccinator, with a staff of officers for the various departments of sanitary work—nuisance removal, epidemic inspection, lodging-house regulation, night inspection of small houses for the repression of over-crowding, etc., etc.

2. Two fever hospitals, one of 130, the other of 330 beds, and one small-pox hospital with 150 beds, all permanent or in process of being made so, as the original pavilions decay, and with ample ground for additional pavilions if needed. The temporary hospitals were built out of the current local rates, but the permanent structures have been erected by loan from the Public Works Loan Commissioners, a rate of 1-8th of a penny on houses under £10 rental, and ¼th above, being levied for interest and repayment in twenty years.

3. A reception-house, for lodging and maintaining in quarantine poor families from infected houses.

4. A washing and disinfecting establishment, with a service of vans for conveyance of clothing, etc., and a staff, according to requirement, for disinfecting and lime-washing houses.

5. An Inspector of Cleansing, and a cleansing staff, supported by separate assessment, but controlled by the Committee of Health, so as to work on proper principles in harmony with the requirements of health.

I have still to explain the policy with which this organisation is administered; because, as I have already said, you may have in a community the most perfect organisation, and yet, if it is not worked so as to secure territorial and functional unity in dealing with infectious disease, it will be shorn of half its efficiency. It is curious and instructive, now that we have surmounted all difficulties and disabused ourselves and others of all prejudices and preconceptions, and finally worked out our policy, to look back upon the incidents, sometimes accidents, which have at times co-operated with our design, at times even prevented us from pursuing courses which were inconsistent with it. The disinfecting and cleansing of houses and clothing soon came to be accepted as a duty to be performed by us within the area of the rates, without special charge in any case. So, with refer-

ence to domestic servants or other members or dependants of the better classes, we used to exact payment for treatment, but this also was soon abandoned. The greatest difficulty in realising our public policy was experienced in our relations with the Royal Infirmary and the Parochial Boards. The difficulty arose, in the former case, from the obligations which the managers owed to their subscribers, leading *them* to resist our efforts to induce them to exclude cases of infectious disease; in the latter case, from the statutory obligations which the Poor-Law imposed upon Parochial Boards, which led *us* to resist their constant endeavour to throw upon us the treatment of paupers. Fortunately, the accommodation of the Infirmary for cases of general sickness became manifestly insufficient. We said, why fill your beds with cases for which there is a statutory provision supported by assessment, to the exclusion of cases which entirely depend upon charity? The eyes of the managers were first opened to the facilities of our policy by the peculiar properties of small-pox, which being treated in certain wards spread to the others, and became a nuisance as well as a public danger. They dropped it as being too hot to hold in 1871. Immediate difficulties arose with orders for admission received for patients living beyond the municipal boundaries. The managers endeavoured to develop a right of patronage over our beds on behalf of subscribers. We said, for subscribers within the bounds we shall charge £1, but for those outside £2. We soon learned that the suburban authorities were finding relief alike to their rates and their small-pox at our expense, through the Infirmary, and refused outside cases on any terms. This difficulty being surmounted in the case of small-pox, the managers, finding their institution no less popular and much more comfortable, and the demands of general sickness increasing, shut their fever-house in 1874. We rather shabbily, as in the case of small-pox, higgled about payment for city cases recommended by subscribers, on the ground that the hospital funds had had the benefit of a subscription; but, after retreating upon a charge for cases from beyond the bounds, we finally settled down to a policy in advance of that adopted in the case of small-pox—refuse to receive any subscriber living outside the area of the rates, but from within all are welcome. A new general hospital, the Western Infirmary, had been opened at this time, boldly announcing that no infectious disease would be admitted. It was none the less generously supported by the working classes, whose patronage the Infirmary managers were afraid of losing, and, since 1876, no cases of infectious disease, ex-

cepting an occasional one of enteric fever, have been admitted. With the Parochial Boards, we had the game in our own hands, because they were endeavouring to coerce us into the true policy; but human nature is always the same, and not until last year did we finally give in. We did so purely, and, for our protection in any future emergency, avowedly, on grounds of public policy, and not as the acknowledgment of a legal obligation. Undoubtedly a pauper is a pauper, whether the disease be infectious or non-infectious. This must be said to our credit, that we never wrangled over the disposal of a patient, leaving that patient, meanwhile, uncared for. For the last ten years not a case of fever has been treated as a pauper within our city. We removed every body to hospital and then raised our claims against the parishes. This put us in the position of claiming payment for doing something, which the party against whom we made the claim never asked us to do. At first they admitted liability after investigation, just as they would in any case of general disease. Then they denied liability, excepting for persons actually on the pauper roll when attacked with infectious disease. Ultimately, we found ourselves keeping up a system of books and ledger accounts which mostly represented bad debts, and caused constant irritation between public bodies in the same community. Last year, therefore, we finally rose to the policy which I have been chiefly anxious in this paper to bring before you—that without distinction of class or social position, within the area of the rates, all the duties concerned in the management of infectious disease should be discharged by the municipal authorities, for the benefit of all, without special charges. Our hospitals are truly republican institutions, within whose walls there are no paupers, no rich, and no poor—only citizens afflicted with disease which may be dangerous to the common weal.

We have thus secured unity of policy within the municipal boundary; but what about our territorial unity? We have seen how, at the end of last century and the beginning of this, our territorial unity was destroyed by the outgrowth of the population and the development of separate jurisdictions, and how it was restored in 1846 by the amalgamation of those jurisdictions into one municipality. Since that date, Glasgow has continued to overflow her boundaries until, at the census of last year, the community was found to number 681,000 persons, of whom 25 per cent. are under the jurisdiction of no less than nine independent Burghs, which have from time to time been constituted during the last

thirty years. This is a fact of serious importance in a community which is essentially one; and that it is a growing evil will be apparent when I state that during the last ten years, while Glasgow added only 4 per cent. to its population, the suburbs added 19 per cent. I shall not enter into any discussion of the general disadvantages attending this condition of territorial subdivision and antagonism, but confine myself to its relations to the subject in hand. The sanitary disadvantages are no doubt the greatest. At many points of the municipal boundary, there is no natural division whatever between us and our neighbours. Houses are cut in two, streets are cut across, and everywhere you pass by a step into another jurisdiction. In my experience, epidemics never cover the whole area of the community at their first incidence. They begin in one district, and if not checked, eat their way through the mass, while if they are vigorously attacked they may be stamped out in that district. If they begin within the municipal limits, the whole repressive force of the municipality is directed upon the spot. If they begin outside those limits, the appliances of the petty Burgh, even at their best, cannot command adequate resources; and we can only stand by to quench such sparks as may be projected into our premises. Still, as regards infectious disease, we have been able to exercise a wholesome influence not only upon our immediate neighbours, but upon the rural authorities for many miles around. The Public Health (Scotland) Act enables the Board of Supervision (the Central Board of Control in Scotch health matters) to compel all local authorities to appoint a Medical Officer and Sanitary Inspector, and whenever we found our interests at stake, and that those officials did not exist, we have directed the attention of the Board to the fact, and procured their appointment. As regards hospital accommodation, the Royal Infirmary was the key of the position. Upon it, as we have seen, not only Glasgow, but the suburban and rural authorities for miles around, depended for fever accommodation. So soon, therefore, as we had provided for ourselves, we set to work to compel all those local authorities to erect hospitals of their own, under the permissive powers of the Public Health Act. The first effect of drafting infectious disease from the wards of the Infirmary to our own was to place more ample accommodation at the disposal of those authorities, or of subscribers living within their jurisdiction. The result of this in the case of small-pox was especially aggravating. It spread in the wards of the Infirmary and infected the city. The recognition of this fact

speedily led, as we have seen, to its total exclusion. Before the managers adopted the same course with regard to fever, the proportion of the total cases received into the Infirmary from beyond the municipal bounds, through subscribers and local authorities, had risen, from year to year, until in 1876 it reached 16 per cent. from the immediate suburbs, and no less than 44 per cent. from the surrounding country. Having finally got into our own hands all the hospital accommodation for infectious disease in the city, it is obvious that in such circumstances we had a very important part to play. We were beset with applications for assistance from authorities and private parties. To the former we said—we shall afford it at a certain rate of charge, provided you undertake immediately to procure hospitals of your own. The latter, we for several years accommodated by private agreement; but we soon found that the local authorities under whom these persons lived were circumventing us in this way. We, therefore, resolved to refer all private applicants to their own authority, with whom alone we would deal. We acted in the same way with requests for the assistance of our disinfecting staff and our washing establishment. Some difficulty was experienced in the case of certain local institutions, such as Reformatories and the Deaf and Dumb Asylum. Their managers argued that they were Glasgow institutions, and this argument was for some time listened to by our authority. Now, however, we refer them all to the authority within whose bounds they stand. So with two of our Poor-houses. In their case the argument was: They are mostly Glasgow paupers, and our parishes, though they extend landward beyond the municipal bounds, are assessed uniformly, and you are robbing Peter to pay Paul if you refuse us or even make us pay as highly as mere foreigners. But we replied: The case is not so. You are the local authority for such parts of your parish as are not burghal. We treat your paupers free of charge so far as your parish lies within our Burgh. Apply to the other Burghs for similar offices; and where you yourself are the local authority, you have distinct powers of assessment as such over that area, and ought to debit yourself in that capacity. As to paupers in your poor-house, as persons suffering from infectious disease, they are chargeable in the place where they presently lie, not to that from which they have been removed. The aggregate result of all these negotiations has been that two suburban hospitals have been erected by the combination of five of the neighbouring Burghs; that the remaining four send their cases to us at a charge of £8 each; that one of our parishes sends its

landward paupers to these hospitals and the other to us at the same charge; that several hospitals have been erected in rural districts; and that in general, when a case of infectious disease occurs, everybody knows where to look for immediate assistance. If a case is found within our bounds, whether resident or peripatetic, we take it at once; if an application is made regarding a person living outside, we refer it to the proper authority, certain that it will be immediately cared for, or, at any rate, that the responsibility is formally brought home to the responsible authority.

Allow me now to give some brief details of the practice which is pursued upon these principles. We have no local system of compulsory reporting of infectious disease. We have long felt this to be a serious defect, but we would prefer that some such system should be instituted by imperial legislation for the whole country. We have always cultivated the friendship of the medical profession, and have been fortunate in securing it hitherto. Although we entertain strong opinions as to compulsory reporting, we choose to avoid, if possible, that local irritation which is so apt to attend its local adoption. The sources of our information at present are: the registers of deaths, transcripts of which are obtained weekly by payment of three halfpence for each entry, but which are also open to the daily scrutiny of our inspectors; the voluntary reporting of practitioners, who are all furnished with printed forms and stamped and addressed envelopes for their transmission; the School Board officers, who also are provided with forms; the private information of neighbours and others. But 49 per cent. of all the cases known to the department are discovered by house-to-house visitation. Whenever a case has come to our knowledge through any channel, every house in the immediate vicinity is visited, and all houses known to be infected are kept under daily surveillance until a fortnight has elapsed since the occurrence of the last case. If, in the course of his visitation, an inspector finds a person ill, the nature of the disease not being apparent or being denied, and especially if no practitioner is in attendance, he is provided with a book of blank orders, one of which he fills up, and delivers to the householder, or directly to some medical man, by preference to the district parochial surgeon, requesting him to visit and sign the attached certificate. For this service, a fee of half-a-crown, which covers any number of cases found in one family at one time, is paid by the Town Council. In any case where removal to hospital is thought to be necessary, and no opposition is offered, the inspector goes to the nearest

district police station. These are all in telegraphic communication with the sanitary office. He transmits the requisite message, and from thence the order for the ambulance is wired to the hospital to which the patient is to be removed.

In the evening, the epidemic inspector returns to the office with the particulars of each case, entered in due form in his memorandum-book, with the measures adopted. These are submitted individually to the Medical Officer, who sees at a glance the sanitary facts, consults and advises as to special features, and makes a short private note of the cases, so as to keep the leading points in continuous relation clearly before his mind from day to day. Before leaving the office, each inspector adopts the measures requisite to have the routine procedures, and any special instructions, carried out. He orders washing of clothes and disinfection of houses; communicates instances of overcrowding, or of nuisance noted, to the proper officers; directs removal to the reception-house of any infected families to whom it has been thought necessary to offer such accommodation for a fourteen days' quarantine, and who have agreed to accept it; and fills in School-board notices. These have for their object (1) the intimation to teachers and to the Board of the existence of infectious disease in families from which children are attending school; (2) intimation that families previously reported are now free of infection. The washing of clothing and the disinfection and lime-washing or size-colouring of houses is deemed to be of supreme importance. The filthiness of the bedding and clothing of the lower orders of Glasgow is simply indescribable. This, taken in connection with facts established regarding the inherent vitality of the contagia, leads us to regard these articles as perennial sources of disease. The germs of infection acquired from one outbreak are stored up from year to year, perpetually springing into renewed activity in the persons of the susceptible, acquiring epidemic virulence under general intermittent conditions which are little understood, and thus perpetually restored; so that, unless the stock is cut short by a thorough washing, there comes no natural end to their lineal propagation. Our experience gives us confidence in the disinfecting efficiency of thorough washing, with boiling, by the injection of steam, without the use of any substance or process commonly called disinfecting, where such washing is applicable. All our washing is carried out in common in one establishment (which has for some time not been commodious enough, but which we are about to rebuild on a much larger scale), and yet we have never known a single instance even of suspicion of the introduction of a new dis-

ease into any house, or of the revival of the old. Against old mattresses and pillows we wage perpetual war. Fortunately, the materials chiefly used in Scotland by the working classes are such that they can be burned and replaced at small cost. They are mostly straw and chaff, or wool-flock of a very inferior quality. For all these reasons it has come to be recognised as a standing order of the department, wherever a dirty house is associated with *any form of infectious disease* which can be made a legal excuse for the expenditure of the rates in such operations, to seize that excuse and start the inmates afresh in life with a thorough purification.

A few figures derived from the records of the department for the ten years 1871-80, will give you some idea of the nature and extent of these measures. During those ten years, 2,529,443 house-to-house visits have been made, between one and two per cent. of which visits have resulted in the discovery of cases of infectious disease. In all, 71,314 cases have been registered and appropriately dealt with, comprising all the varieties found in this country. Of these, 25 per cent. have been removed to hospital. The hospital year does not coincide with the departmental year; but, in the ten years from 1st May 1871 to 30th April 1881, we have treated 19,595 cases; comprising 4,118 cases of scarlet fever, 3,560 of enteric fever, 3,378 of typhus, 3,122 of small-pox, 1,820 of relapsing fever, 1,138 of measles, 220 of whooping-cough, besides sundry cases of diphtheria and erysipelas, which, with 413 classified as febricula, and 1,833 mothers admitted to nurse their own children, or persons suffering from acute diseases other than fever, make up a grand total of 19,595. In our "Reception House" we have received 2,554 persons from infected houses. Since our hospitals were established in April 1865, upwards of 30,000 persons have passed through our wards; besides those treated during their erection and extension, at our expense, by the Parishes and the Royal Infirmary.

There are some special points in the management of individual epidemic diseases which merit a short reference. There is small-pox, of which you have considerable experience in London. The prime point of preventive practice is, of course, vaccination. By one of those unfortunate divisions of responsibility, which are inevitable in the course of piece-meal legislation, before the vision is enlarged so as to take in the whole horizon of the object which ought to be in view; the practical enforcement of the Scotch Vaccination Act is entrusted to the Parochial Boards. I early learned that it is impossible to induce persons to submit to re-vacci-

nation except in presence of small-pox; but it has always been a standing order to the sanitary staff to be on the outlook for those who had escaped primary vaccination, with whom we could deal not wholly by favour or persuasion on the ground of personal advantage. In 1875, I discovered that fully 3 per cent. of all the children born in Glasgow were returned as unvaccinated; so that in seven years we had produced between four and five thousand who, so far as known, were unprotected—a fact of serious importance, not only for us but for the country at large. I found, on investigation, that while all the statutory forms were duly observed by the Inspectors of Poor, there was no special effort, certainly no thorough method of personal search, instituted for the discovery of defaulters. I therefore made a private arrangement with the inspector of our largest parish, in which the work seemed to receive most attention, to have transmitted to me monthly a list of the children who had been reported by the Registrars, and of whom the parochial officials had been satisfied they could find no trace. I distributed those names among the district sanitary inspectors, with this result: that they successfully followed up and found fully 26 per cent. of those children. The majority were found to have been already vaccinated, although the certificate had not been lodged; but 6 per cent. of those reported to me, and who were found, had not been vaccinated. This gratifying result induced me to ask the Town Council to authorise me to make this a permanent branch of our work, and so, by voluntary effort, to remedy the division of responsibility created by law. The permission asked was at once granted; and we now obtain weekly transcripts of the vaccination register from the registrars. The natural unity of this service with general sanitary work, is demonstrated by the fact that we overtake it without adding a single man to our staff. There is no such thing in Scotland, to the credit of our national intelligence, as an organised obstruction to the operations of the Vaccination Act. Among the defaulters, I have only encountered two persons in six years who had objections to the use of humanised lymph; and for these I at once obtained vaccine lymph, as I am prepared to do in any case where that will remove any difficulty. The defaulters belong, with hardly an exception, to that class of the population who are negligent of all duties, whether legal or moral, who are vicious, irregular, ignorant, or, at the least, thoughtless and unfortunate; 26 per cent. of the children involved were illegitimate. It is among that class our staff is constantly moving, and with them they are in constant

relation, so that, in the course of their ordinary house-to-house visits they take up this with their other duties.

As to re-vaccination, as already stated, it is no use endeavouring to persuade the poor to be rational, and submit to this operation as a routine practice, in anticipation of risks which they cannot discover. In presence of small-pox they will submit, but only if the operation can be effected at once. At first we acted in this way. On visiting, for the first time, a case of small-pox, the inspector was instructed to examine all those in the house and neighbourhood, and secure the names of those who seemed to require protection. A medical man was then sent to the locality with this list; but our invariable experience was that, in the interval, there had been a general laying of heads together, and consequent consolidation of all prejudices and old wives' fables, so that scarcely anybody would submit. Here was a practical difficulty to be overcome; and the common-sense view of the dangers to be avoided was this. Provided the lymph is carefully selected by a competent medical vaccinator, any intelligent layman instructed by this vaccinator is as well able to perform the mere operation of re-vaccination, and to judge, according to easily defined rules, on whom it ought to be performed, as any medical man can be. Therefore, I had each epidemic officer put through a short course of instruction in the operation. We have always had a station for primary vaccination, managed by a medical vaccinator. He selected the lymph and was responsible for its purity. Tubes were charged by him, and each collection numbered, so that its origin and its pedigree were known. The inspectors always carried a stock of this lymph about with them. Whenever one of them went into an infected house, he said, "Now, you know, you must all be re-vaccinated." Arms were bared and the thing was done before they had time to think about it. I do not mean to say that no recalcitrants were encountered, but I do say that a degree of success was obtained in this way which would not have been possible in any other way. Where the parties had a private medical attendant, the inspector advised them to go to him, told the doctor of the circumstances, gave him lymph if he required it, and did not lose sight of the family until the operation was done. Primary vaccinations were not done by the inspectors, but a note was made of unprotected children, and the mothers either took them to the station or the medical vaccinator went to them. As to cases of small-pox, we enforced removal to hospital with much more sternness than in the case of any other infectious

disease. We had no hesitation in applying for magisterial warrants when persuasion failed, but after executing one in a locality it was seldom necessary again to appeal to force. In the ten years 1871-80, we performed 12,718 primary, and 9,614 secondary vaccinations. I believe the latter are understated, as under pressure the men were more careful to get as many protected as possible than to record their cases. The practical result may be summed up thus. Small-pox was epidemic during the four years 1871-2-3-4, during which time 4,328 cases were known to exist, of which 74 per cent. were treated in hospital, and 8,730 re-vaccinations were recorded as done by our own staff, or an average of fully two persons for each case. The total deaths in three years numbered 786, of which 67 per cent. occurred in hospital. The death-rate was never higher than 4.3 per 10,000 of our population, and averaged 3.8. Since 1875, when small-pox caused only two deaths in Glasgow, it has been in entire abeyance, there having been only twenty-five deaths in seven years. I desire to leave to others the making of comparisons; but it is only fair to say, on the one hand, that the Scotch towns have all in recent years been almost, but not quite, as free from small-pox as Glasgow; and, on the other hand, that while the epidemic acme of Glasgow was passed with only 4.3 deaths per 10,000 inhabitants, in 1873; it reached 46.7 per 10,000 in Leith, in 1872; 35.3, in 1872, in Edinburgh; 32, in 1873, in Greenock; 24.2, in 1872, in Dundee; 14.5, in 1872, in Aberdeen; 12.5, in 1874, in Paisley; and 6.5, in 1872, in Perth.

Typhus.—Overcrowding of inmates in dwelling-houses, with the personal filthiness which almost invariably accompanies overcrowding, are especially favourable to the development of epidemic typhus. Thirty per cent. of the dwellings in Glasgow consist of only single apartments, 44 per cent. of two, and 15 per cent. of three apartments. There are special clauses in our local Police Act enabling us to measure the cubic contents of all such dwellings, and to affix upon them a ticket stating the number of inmates allowed, on the very moderate scale of 300 cubic feet for each adult, two children under eight being reckoned as one. We propose to raise this standard to 400 cubic feet in our new Police Act. This ticketing is accompanied with the right of night-inspection, for the purpose of counting the inmates; overcrowding subjecting the householder to a penalty. We regard the constant exercise of this right of supervision as our main protection against typhus. At first it was carried out by the police, but in that and other respects we soon cut our

connection with the police. Night after night, all the year round, our inspectors are out in some part of the city. Their inspections have averaged over 41,000 a year for the last ten years, in only 4 per cent. of which was overcrowding discovered. In 1866, when typhus was epidemic, Dr. Gairdner found that 8 per cent. of the houses visited were overcrowded, and that the epidemic incidence varied in different districts almost in exact proportion to the overcrowding discovered. Previous to 1873, typhus was not distinguished in the registers of deaths from other "fevers", so that I cannot contrast its fatality over the same periods as that of small-pox. But in the five years, 1876-80, there were only 258 deaths from typhus, or a death-rate of barely 1 per 10,000 of the population (.99 exactly). In 1881 it was only .82 per 10,000. The deaths ascribed to "fever" in those years of epidemic prevalence, to which reference has been made in the course of this paper, were undoubtedly almost entirely caused by typhus; but, to make the comparison quite unimpeachable, I may state that in 1871, the death-rate from all "fevers" was 16 per 10,000; that it has fallen year by year, with only one slight break in 1880, caused by an epidemic of enteric fever, introduced through the medium of milk sent in from a farm twenty miles distant, and that in 1881 it was only 4¼ (4.34) per 10,000. In the ten years, 1871-80, we have a mean death-rate of only 7 per 10,000, against a mean of 20¼ (20.24) in the ten years immediately preceding. In the great epidemic of 1837, it was estimated that the death-rate was 86 per 10,000 from "fever", and in the five years, 1837-41, it maintained a mean of no less than 44½. In the last ten years, we have treated in our hospitals 3,378 cases of typhus; while in 1837, the city having only half its present population, and the cases isolated forming only a moiety of those which existed, there were 5,387 cases treated in the Infirmary.

The infectious diseases which chiefly affect children form a large and very fatal class, which, amid all the advances of modern sanitation, still rival in their epidemic outbursts those infectious diseases of adults whose periodic ravages have almost become events of the past. The great obstacle in their preventive treatment is the fact that they do affect children chiefly. The feelings of the parents will always stand in the way of isolation. I believe, however, that our systematic application of washing and disinfection of the houses has been eminently beneficial, removing from the household those perennial sources of infection revived in the persons of successive susceptible generations of children.

We have also endeavoured gently and persistently to familiarise parents with hospital treatment. The admission of mothers along with children of tender years has been a great inducement. Very frequently, after remaining a few days, they acquire confidence in the management of the nurses, and leave the patients voluntarily to their care. The benefit to be derived from isolation varies in the different diseases. In scarlet fever it is at its maximum. During the dangerous period of convalescence a control is exercised which is impossible at the hands of parents, except in rare circumstances. We find that an average residence of about eight weeks is requisite; but with every precaution, there is no contagium which is so adhesive, and so apt to linger in unexpected corners of the person and clothing, as that of scarlet fever. We find that the dismission of patients is very difficult to arrange, so as to clearly cut off the infection and leave it behind. Parents send in clothing, or the disinfected clothing is got out of store. They promise to call on a certain day at a certain hour, and the child is dressed in anticipation. They do not come, and then mischief is very likely to ensue. We are endeavouring to overcome these difficulties by something which might be called a convalescent clearing-house, but it is not easy to cover all eventualities even then. Hospital treatment of measles is of minimum use, except as saving life by the superior care and medical skill bestowed upon the patients admitted. The infection is scattered abroad before the child is removed. Therefore, we pay especial attention to the exclusion from schools of children who belong to infected families. Whooping-cough occupies much the same position. It is an interminable disease, tapering off at both ends into a condition not recognisable except by relation to the condition which supervenes or precedes. The child is on the whole vigorous, except in the youngest cases. I believe, therefore, that the benefit of treatment to the individual case is almost all that can be claimed for the hospital treatment of whooping-cough. In Glasgow, where pulmonary diseases are so rife and so fatal, it is a fearful scourge. In 1872, it caused a death-rate of 21 per 10,000 of the entire population. In 1874, scarlet fever produced a death-rate of 33 per 10,000, and measles, in 1871, of 18. Still, contrasting the ten years, 1861-70, with the ten years, 1871-80, we find in all these diseases substantial evidence of improvement—in the case of scarlet fever, a fall from a death-rate of 13 per 10,000 to one of 10; in measles, from 8 to 7; in whooping-cough, from 15 to 12½.

I wish, in conclusion, merely to point out that during the

ten years to which I have appealed for the results of the policy and practice of Glasgow in the management of epidemic disease, both the policy and the practice were only being matured and developed; more especially, the policy did not reach its full proportions until the last years of the decade. I think, therefore, it is pretty safe to anticipate that whoever may sum up the results of the ten years which now lie immediately in the future of Glasgow will have some facts to put in evidence which will tell still more strongly and decidedly in favour of the policy and practice which I have ventured to bring before the Epidemiological Society of London.

ON THE FORM OF THE EPIDEMIC WAVE, AND SOME OF ITS PROBABLE CAUSES.

By ARTHUR RANSOME, M.D.

(*Read: June 7th*, 1882.)

The course of an epidemic disease through a country may aptly be compared to a wave, gradually rising and then falling, with more or less regularity, again to rise after a period which varies indeed, but which, in the same disease, is sufficiently regular to entitle it to comparison with "a wave upon the enridged sea".

1. When we survey the track taken by any of the more common and fatal of the infectious diseases, throughout a long series of years, we cannot but be struck with the fact that they observe definite periodic times or cycles. The periods differ for the several complaints, but as I have shown, in a paper "On Epidemic Cycles",* in a sparsely populated country like Sweden, they are remarkably constant for the same disease, and may fairly be spoken of as a succession of waves.

2. In some diseases, such as scarlet fever and whooping-cough, and perhaps also small-pox, there are signs of still more wide-spread variations in the course of these complaints.

A long series of years is needed to display them, but in the hundred years or more afforded by the Swedish tables of mortality (see Charts in Appendix), I think we may discover certain larger as well as smaller disease-waves. In the scarlet fever curve (Chart I), for instance, not only a short cycle of four to six years may be traced, but also a long undulation of fifteen or twenty years or more, which may be likened to a vast wave of disease upon which the lesser epidemics show like ripples upon the surface of an ocean swell.

In whooping-cough, again, in Sweden (Chart II), between the years 1775 and 1825, there appears to be a fifty years' period, in the first half of which there is a constantly increasing height of the epidemic wave, together with an equally constant lowering of the rate of mortality in the inter-epidemic

* *Proc. of Lit. and Phil. Soc.*, Manchester, Jan. 27, 1880.

periods. Then, in the second part of the time, namely, from 1803 to 1825, there is a gradual rise in the amount of disease constantly present, and a corresponding lessening of the height of the more important of the successive epidemics.

In small-pox, traces of the same phenomenon may perhaps be found in these Swedish tables (Chart III), between the years 1750 and 1773, and between this latter year and 1795. After this date the influence of vaccination has been such as to destroy entirely the natural course of the epidemic. No such large variation is to be observed in the Chart (IV) of Measles.

But though these Swedish records give us a good idea of the great movements of epidemic diseases, it is difficult to gain from them an idea of the actual shape of the curves described by an advancing or a receding epidemic. An annual return is far too comprehensive to show traces of the minor variations in the track of the disease. This can only be gathered from a record of the number of cases occurring, at the longest, each month, or, better still, each week of its duration. Moreover, mortality tables, even when thrown into the graphical form, are hardly suited for our purpose.

The number of deaths, even when they are recorded every week, gives a very imperfect view of the course which the complaint is taking. The fatality of different epidemics varies very greatly; at one time they may be so malignant, that they kill a large proportion of those attacked; at another they may be so mild that the deaths are comparatively very few. Moreover, an epidemic notoriously varies in its virulence during the whole period of its visitation. At its outset, for instance, it is usually much more fatal than at its close.

If we add to these considerations the uncertainty as to the date at which the death occurs after the outbreak of the disease, and the chance that the record of the fatal result is made at a distance from the place where the disease was contracted—when all these objections to the use of mortality tables are weighed, I think that we shall see why any representation of the epidemic wave, derived from death records only, must give a very imperfect idea of the actual track followed by the disease, and of the times at which the variations in its course occurred.

Nor can we speak more favourably of the plan adopted by Professor von Pettenkofer for obtaining an average curve of cholera. In this method, all the cases and all the deaths that have occurred in Prussia during each fortnight of the year, for twelve years, are added together respectively, and

thus a kind of average of sickness and mortality is struck which, indeed, shows the marvellous regularity of this disease in Prussia, and its predilection for certain months in the year, but it cannot pass for the natural course of the epidemic in other places.

In Professor von Pettenkofer's own account of the cholera in Bavaria in 1873, and especially at the convict prison of Laufen, it is evident that at Munich the disease was at a high point in August, and that after this there was a distinct lull in the complaint until the months of December and January.

From Mr. Netten Radcliffe's account of cholera also, in the report of the Medical Officer to the Privy Council for 1866, it is evident that the disease may prevail at any season. Any attempt, therefore, at massing the figures of several epidemic years must certainly defeat its own object.

For a thorough investigation of this subject we need, indeed, a record of every case of infectious disease as it arises in the community, and an indication of its habitat. Hitherto such a record has never been made, but we may reasonably hope that in the course of a few years, our requirements will be fulfilled by the publication of the returns now furnished in many towns, under the system of a compulsory notification of infectious disease. No such source of information is at present available for our purpose, but it is possible that something short of this perfection may suffice to enable us to obtain an idea of the forms assumed by epidemic waves passing through limited areas of population.

In the weekly returns of disease, published by the Manchester and Salford Sanitary Association, we have a very fair representation of the rise and fall of diseases in those towns. For nearly twenty years, from 1860 to 1880, these returns were made with great regularity from a certain number of poor-law districts, hospitals, workhouses, and other public institutions, and, seeing that during this period, the greatest care was taken to obtain the required information not only regularly, but from the same constituencies, without varying the sources of the reports, we may take them, with much confidence as our guides in this inquiry.

For the first ten years of this publication the records were kept in their greatest perfection, and the results are given in a graphical form in the Charts of Disease, published by the Association in the year 1872.* Since that time there has been less regularity in their publication, and they were

* These Charts are to be obtained at the offices of the Sanitary Association, 100, King Street, Manchester.

finally abandoned in the year 1880 on account of the difficulty of preserving the regularity of the returns. In the period comprised within the above-mentioned years there have been numerous epidemics of scarlet fever, small-pox, measles, whooping-cough, and the continued fevers, and the curves delineating some of these disease-storms may be useful to us in our inquiry, even though they cannot be regarded as entirely sufficient for the purpose.

We will now take these charts as a first basis from which to describe the epidemic curve, and we find from them, in the first place, as might have been anticipated, considerable diversity in the course of the several disorders. We may also note the irregularity of the track that they all follow from week to week.

3. This is the third point to be noted respecting the waves of epidemic disease. In few cases, whether they are ascending or descending, do they pursue an even course. The most general track is a series of zig-zags, leading, on the whole, up or down as the case may be. It may be permissible, however, to take the mean of the numbers recorded, and thus to imagine an ideal curve with a less wavering course; or the monthly returns may occasionally be used, giving a steadier average, and when this is done we find a much closer likeness between the curves of the several epidemic waves; and we may note in them several points of interest, and such of them as call for subsequent remark, I propose to denote by figures consecutive to those already given.

Before, however, passing on to the curves of the epidemics themselves, it is perhaps worthy of observation that few of these diseases are ever entirely absent from the town. Single cases crop up throughout the whole of even the non-epidemic period, and show that there are sources of infection continually present in some part or other of the place. The longest period throughout which a disease is entirely absent is nine or ten weeks (from March 14th to May 23rd, 1868) in the case of small-pox. Whether or not this constant presence of disease is due to the existence of "fever-nests or other foci of disease", or whether—as in the case of scarlet fever seems possible—bad drainage may account for its existence, it is sufficient for us here to note that there is no interval that could not be bridged over by infection either directly from the person or indirectly from some infected medium.

Continuing, now, our remarks upon the epidemic wave itself we notice :—

4. In almost every case in which an epidemic is impend-

ing, for some weeks or months before its decided outbreak, that there is a general slight increase of the disease, that may be compared to the low muttering that preludes a coming storm. This preliminary period varies very much. In the epidemic of scarlet fever of 1868, it extended to eight months (from September 1867 to May 1868). In 1862, in anticipation of a small-pox storm, it lasted from May to November; and in 1869 and 1870 more than two years. It is noticeable that, except at the times of great epidemics, the curves of small-pox are always most irregular; and after this disease must rank measles. In whooping-cough, also, the initial period is always comparatively short.

5. Even after the sub-epidemic period, as it may be called, the disease seldom leaps at once to its highest point without a break. There is often a decided rise for a time, and then a fall for several weeks, and this may be repeated twice or thrice before the final ascent is made. The curve thus often comes to resemble a mountain-chain, with its highest point surrounded by lesser attendant peaks.

6. In the descent of the epidemic similar variations in its track are often observable, but, on the whole, the decline is usually more rapid than the rise, the second descending portion of the curve from its highest point often being shorter than its ascending part. This fact is most clearly shown in the monthly returns of disease, given in a paper on *Disease in St. Marylebone and Manchester*, published by the Manchester and Salford Sanitary Association in 1866.

7. Lastly, we remark that in several diseases, notably scarlet fever, whooping-cough, and measles, the highest point of the wave occurs at one particular time of the year, peculiar to each disorder.

I do not set down these remarks on the course of infectious diseases in Manchester and Salford, as necessarily applicable to the epidemic wave in all other places. They are, however, borne out in the main by the few other records of a similar kind that we have had from other towns, from Preston, Birmingham, Newcastle-on-Tyne, and St. Marylebone. I here exhibit a diagram showing a succession of epidemics in one year in Preston. So far as it extends, it shows most of the points that have been mentioned above; and the same may be said of the returns from Birmingham, in which town epidemics of measles and whooping-cough (of which I give a diagram) display similar peculiarities. The St. Marylebone records were continued for a much longer period, and I have already called attention to the remarkable likeness between the curves of disease at this place, and in Manchester

and Salford, a likeness extending not only to the general shapes of the curves, but even to many of the lesser undulations, and to the very dates of the highest and lowest points of the epidemics.

We come next to an attempt to account for some of the peculiarities that have been noted in the epidemic wave.

1. I may confess at once that I am unable to give any adequate explanation of the curious phenomenon of the many years' variation of prevalence of epidemics, their "nutation period" as it might be called. Possibly, the fifteen or twenty years' swell of scarlet fever may have something to do with the greater liability of adults to this than to the other epidemics that have been mentioned; but no such hint at an explanation can be given of the fifty years' variation in whooping-cough in Sweden, nor of the similar, though less evident, variation in the mode of attack by small-pox in that country. I have in vain tried to connect these nutations with the sun-spot period, or with any other of the meteorological changes.*

2. With regard to the regular succession of epidemic waves in these diseases we are, I think, in better case. In a paper on "Epidemic Cycles", read before the Manchester and Salford Literary and Philosophical Society last year, I have ventured to account for them on the supposition that before an epidemic can take place, it is necessary that there should be present in the community a sufficient density of the population at the ages susceptible to the disease. To quote from this paper: "Probably all the facts would be accounted for if we suppose that these disorders can only become epidemic when the proximity between susceptible persons becomes sufficiently close for the infection to pass freely from one to another". Exanthematous diseases rarely attack the same individual twice in a lifetime. When, therefore, an epidemic has, by either a fatal or non-fatal attack, cleared away nearly all the susceptible persons in a population—mostly infants and children up to a certain age—then it must necessarily wait a certain number of years before the requisite nearness of susceptible individuals has been again secured. There must in the interval be a gradual restocking of the nation with material fit for the epidemic to feed upon, and it can only spread when the requisite proximity is attained, "when the meshes of the network of communication are sufficiently close, for it to include all susceptible persons in one grand

* A diagram of the sun-spot variations for 100 years (Chart V) is given in the Appendix; but there is to be discovered no simple relation between it and the Charts of Disease.

haul;" or, as it might perhaps be phrased, when the necessary degree of potency of the poison has been obtained to enable it to traverse the average distance between person and person. I must refer my readers to the paper itself for the grounds upon which this theory is based.

3. The next point to be considered is the cause of the irregularity of the epidemic curve. At first glance, it appears hardly surprising that there should be considerable oscillations in the course of these diseases, and it might be supposed that it would be sufficiently accounted for, by the supposition that at irregular intervals the complaint would light upon some fresh collection of susceptible material, that this would blaze up for a time, and then die out as soon as it was exhausted. It is, indeed, highly probable that this is at least one important explanation of the phenomenon, and, in order to make the fact more apparent, I have analysed two of the epidemics of our series; one of small-pox in the years 1871-72; and another of scarlet fever in the years 1863-64. Chart VI in the Appendix displays the course of this last epidemic. I have shown how each of these diseases distributed itself in time throughout the different townships from which our returns were received, and in doing this I have taken into account the relative populations of these districts. In these diagrams we can, to some slight extent, trace the spreading of the disease from one quarter of the town to the other; thus, in the case of small-pox, it commenced in Manchester and Chorlton; thence spread to Salford; afterwards to Ancoats; and, finally, to Hulme. The scarlet fever epidemic also commenced in Manchester (Chart VI, fig. 1), and almost at the same time in Ancoats (fig. 5); then it appeared in an epidemic form after three months' interval in Chorlton (fig. 4); in another two or three months it spread into Hulme (fig. 3); and, finally, after one or two minor assaults, it became established in Salford (fig. 2). It is not a little remarkable also to note the irregular manner in which each of these diseases was meted out to the several districts. The oldest and densest part of the town—that of Ancoats—was the least visited by small-pox, whilst in Chorlton, a comparatively well-built, and more recently constructed district, there were large numbers of persons attacked; and in Salford (fig. 2), another old and crowded district, scarlet fever spread with much less violence than in Chorlton (fig. 4). These charts, will serve my purpose of displaying the irregularity of the course of the disease in different parts of the town, and the mode in which fresh portions of the population were from time to time made its victims. The mere spreading of infec-

tion will then, probably, cover some of the sources of the irregularities in the course of an epidemic.

Again, we might suppose that certain unsanitary conditions in the several parts of the town might vary from time to time, and produce some retarding or favouring influence upon the disease, and this also, in the case of scarlet fever at least, is a possible factor in the result. If, however, we compare the curves of these epidemics in Manchester and Salford, with those of St. Marylebone,* it will become evident that these explanations do not cover all the facts. In these two places there is a remarkable correspondence between even the lesser undulations of the waves of disease; and, as we have already seen, the curves often touch their highest and lowest levels together in point of time. This relation between the curves is perhaps best seen in scarlet fever and small-pox in their epidemic periods, and shows less conspicuously in whooping-cough and measles. Yet it will be found that in both diseases 66 per cent. of the fluctuations of the disease rise and fall at these two places in exact accordance with each other. When we consider the differing influences of position, climate, manufactures and occupations bearing upon the inhabitants of these places, so far distant from one another, it becomes evident that neither simple contagion nor yet fluctuations in sanitary conditions can account for all the irregularities in the track of the epidemic. We are bound to seek, then, for some other explanation of the fluctuations in the course of epidemics that shall be common in two places as far distant, and as different in many ways, as London and Manchester. We shall find it, I think, in the influence of atmospheric changes upon these diseases.

In a paper on this subject, read before the Literary and Philosophical Society of Manchester in the year 1860, based upon the returns of the General Board of Health in London, I pointed out that, "in its chief undulations the measles-curve seems to rise with the fall of the temperature, and *vice versâ;* and that the influence of this element is best marked when it is above or below the average." Whooping-cough also seemed to be influenced by much the same conditions, although the relationship between the two diseases is by no means exact, perhaps owing to the influence of the degree of humidity of the air, which chiefly affected the former complaint (measles). With respect to scarlet fever, it was remarked that "a large amount of aqueous vapour in the air appears greatly to facilitate the formation and action of the peculiar scarlatinal poison, especially when this element is

* *Loc. cit*, p. 100.

accompanied by sudden fluctuations in the atmospheric pressure—a diminished pressure being favourable to the disease".

It is only just to remark that neither in these researches nor in subsequent ones have I been able definitely to trace the influence of atmosphere elements upon the course of small-pox, although it is probably favoured by warmth; and yet this disease is the one that shows the closest correspondence in its variations at London and Manchester. In the epidemic of 1863, 76 per cent. of the undulations were in accordance at the two places; and in 1870-71, 61 per cent. rose and fell together. Whether the meteorological elements affect these disorders directly or indirectly, by producing conditions of the body favourable to their attack, would not materially affect our conclusions—perhaps both these causes are at work; thus, it is impossible to compare the curves of measles and whooping-cough with those of bronchitis and catarrh without being struck by their coincidence in most of the epidemics; and we are almost constrained to admit that the local injuries caused by the latter complaints permitted the entrance of the zymotic element.

In scarlet fever, again, we may perhaps discern some connection between the prevalence of this disease in the autumn and spring, and the occurrence of ulcerative tonsillitis at these times. I am inclined, in both these instances, however, to think that defects of drainage are the connecting link; and I would note the fact of the influence of a falling or low barometer upon scarlet fever as an important argument in favour of Dr. A. Carpenter's view of the conveyance of scarlet fever by means of drains.

It is certainly difficult, however, to attribute the variations of small-pox either to local weakness or to seasonal changes.

Dr. Moore of Dublin, from mortality returns, thought that the disease was most common in the winter, and he was inclined to ascribe this occurrence to the greater crowding together of the poor in their dwellings at this season; but our tables of sickness teach us that the great spread of the epidemics, both in London and Manchester, took place in the early summer, though not at the time of the greatest heat. We shall have to wait for some other explanation of the epidemic constitution in the case of this disease.

4. The sub-epidemic period in these complaints is probably in some way explicable by the necessity for a certain intensity of the poison producing them, or else by the relation between this factor and the density of the susceptible population. We may suppose that the first persons to take the complaint will be those who are peculiarly susceptible to its influence,

but it will not attack those who are less disposed to it; in other words, it will not become a true epidemic until either the poisonous emanations become more concentrated by its preliminary attacks, or, more likely still, until, by the establishment of many centres of infection, the complaint is brought within striking distance of the less susceptible individuals. This part of the subject is, therefore, closely related to the causes of the periodic times observed by epidemics.

5. We have seen that epidemics, after spreading rapidly, subside for a time, and then either rise still higher, or else show one or more lesser but considerable fluctuations on their descending track. I think that this point is of much interest in regard to M. Pasteur's recent observations upon the means by which a zymogen of lowered intensity may again regain its virulent power. He has demonstrated the fact that if an attenuated virus can be made to destroy some very young or enfeebled animal, the infection from this animal will again attack others with all its former virulence. It seems to me of the utmost importance to ascertain whether the renewal of epidemic power in the case of these diseases has been preceded by the deaths of either very young children or enfeebled persons. This observation could probably be made without difficulty by Medical Officers of Health in those towns in which powers have been obtained for the compulsory notification of infectious disease. It would have been very interesting, for instance, to learn whether the secondary epidemic of small-pox in 1865, in Manchester, had been preceded by such deaths as would cause a re-establishment of the virulence of the poison.

6. We now approach the most difficult part of our task, namely, the attempt to account for the general form of the epidemic wave. If we could assume that all the susceptible individuals in a district are attacked, and that the disease spreads from one to another in a series of concentric circles, like a spark spreading in tinder; it would not be difficult to calculate the curve that would be assumed by an epidemic wave in any district of which the boundaries were known. This would be a simple mathematical problem; and even if several district centres of infection were established, although this circumstance would complicate the problem, a solution would probably be attainable with a fair degree of accuracy. It is likely, indeed, that some such mode of spreading of an epidemic does take place; and it is interesting in this regard to compare the curve of the cattle plague in 1865-66, as given by Dr. Farr, with that drawn upon the above hypothesis by my

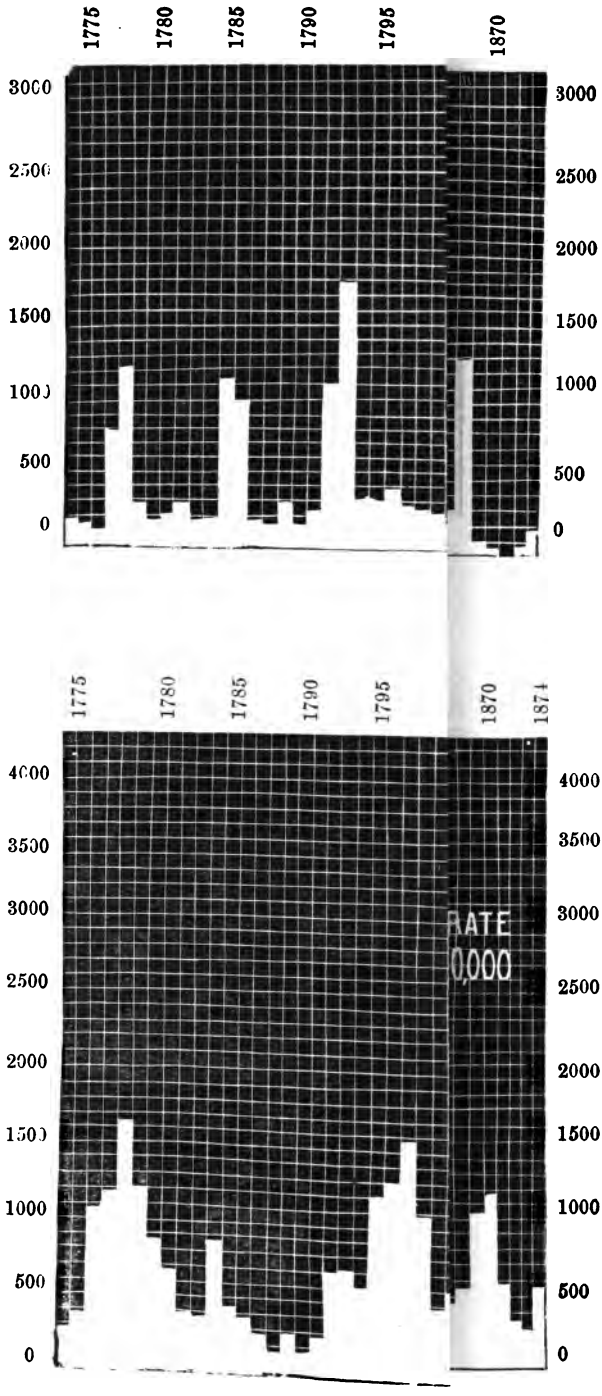
friend, Mr. Alfred Nield.* Still, I do not think the simile of an epidemic spreading like a spark in tinder affords a sufficiently close analogy to what actually takes place. The disease would probably, in this case, blaze up more fiercely, and descend more rapidly, than we have seen that it does. Moreover, it is scarcely likely under this hypothesis that the curves of disease would be so much alike as they actually are found to be, in two districts so dissimilar in shape as are St. Marylebone and Manchester.

Dr. Farr has himself proposed a theory to account for the general shape of the epidemic wave, which seems to me a more probable one, and more in accordance with recent discoveries respecting infection.

In a short paper, written at the time of the great cattle plague—of which unfortunately I have not been able to obtain a copy, and to which therefore I cannot refer—he shows that in most epidemics there is a certain progressive diminution in the rate of infection, a diminution that was sufficiently steady to enable him, in the case of the cattle plague, to take as the basis of his calculation the mortality from this disease in the first few weeks of its course, and from these to predict the date at which it would attain its height and its progress during the decline. I now present a diagram, drawn from the figures given by him at the time, and I remember that I then noted the correctness of his deductions, and that they were unaffected by the "stamping out" process that was then pursued.

This theory is quite in accordance with our knowledge of the diminishing fatality of an epidemic after its first onset; and the recent observations of MM. Pasteur and Toussaint, Dr. Greenfield and others, throw a new light upon the subject, and render it still more probable that in passing through the bodies of those who are successively attacked there is a distinct attenuation of the virus, a degree of attenuation that may perhaps in each epidemic permit of calculation, and enable us to predict its probable course. Only, as I have already pointed out, our calculations are at any time liable to be upset by the additional potency that the virus may obtain in passing through the bodies of young or enfeebled persons. On the whole, however, I am inclined to think that in any attempt at an explanation of the shape of the epidemic wave we shall have to rely mainly upon a knowledge of the changes in the degree of virulence of the epidemic poison.

* See Chart appended to paper "On Epidemics Studied by Means of Statistics of Disease."—*Brit. Med. Journal*, Oct. 10th, 1868, p. 386.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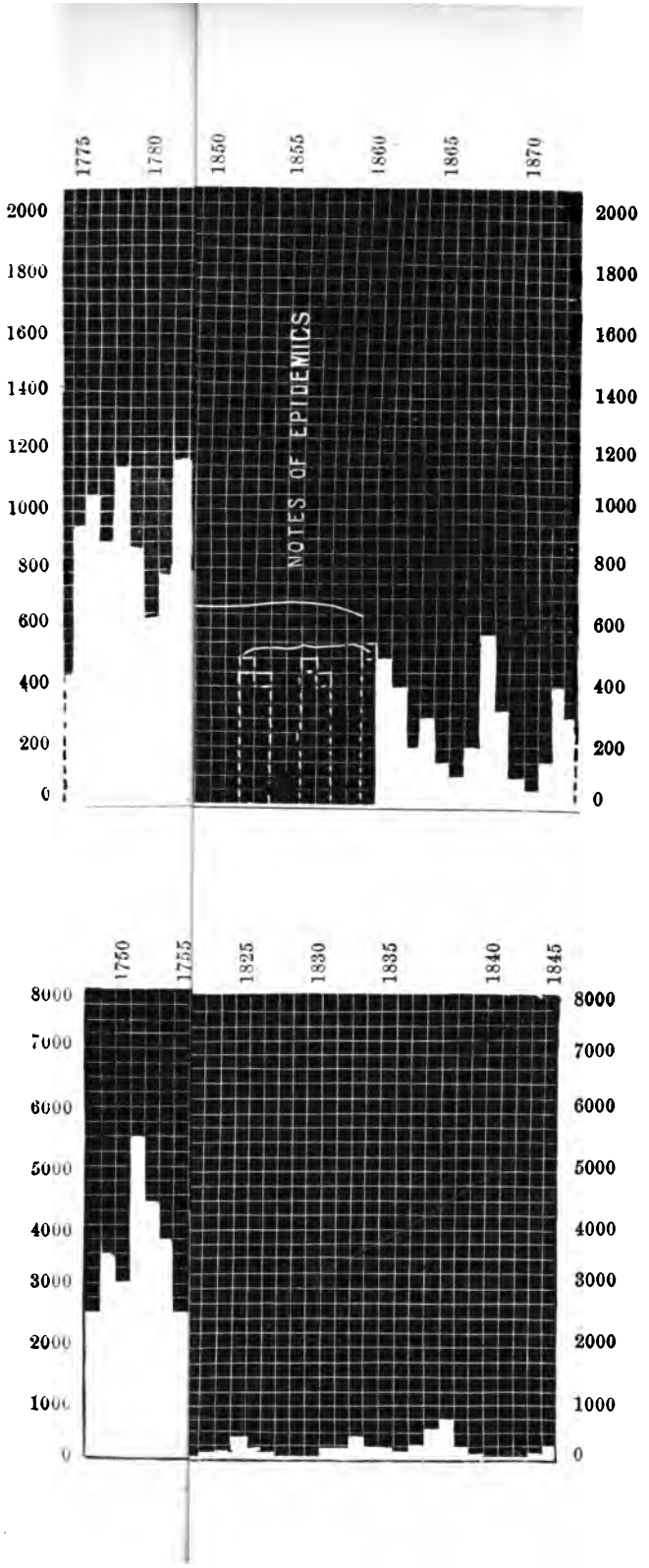
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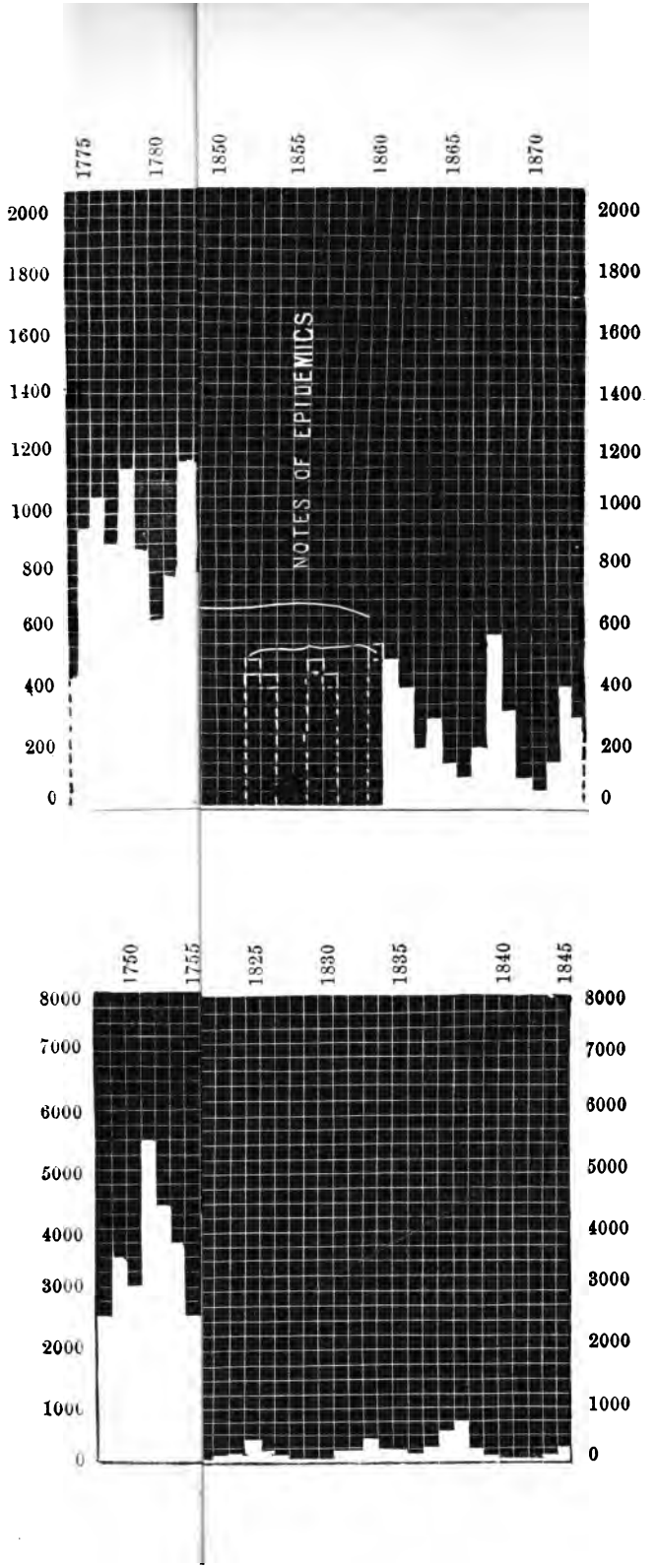
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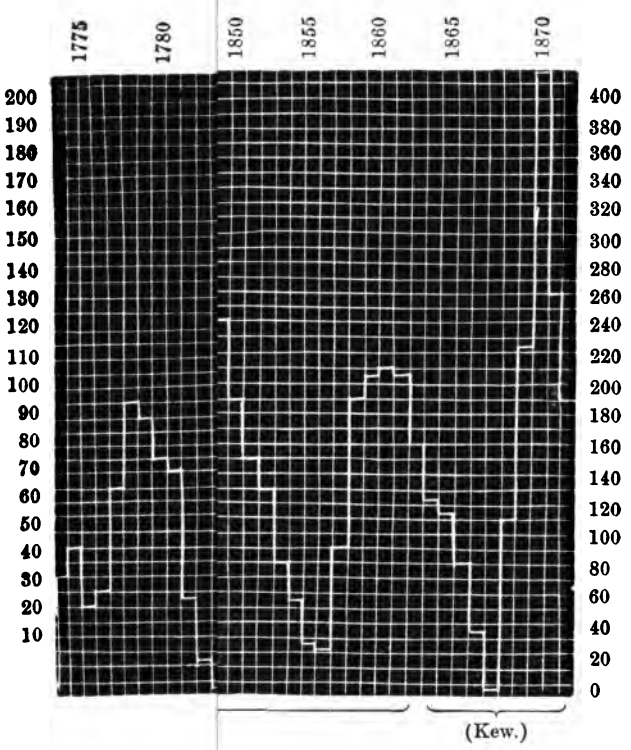
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7. Lastly, we have noted the coincidence of the culminating points of the curves with certain times of the year. This fact again must be associated with the influence of seasonal and atmospheric changes, which have been seen to be operative in producing the lesser undulations of the curves. It is not necessary to bring forward further proofs of the fact that scarlet fever usually touches its highest point, both in epidemic and non-epidemic times, in the autumn; and that it also often again rises somewhat in the spring, or that measles and whooping-cough are usually at their highest in the colder months of the year.

I would, however, in conclusion, again draw attention to the probability that the influence of season is due to its preparing the system for the reception of disease. That there is often a direct infection of the part that is principally affected by a contagious disease, and that this infection is facilitated by the previous weakening of the part by some climatic disorder.

If this observation be correct, it not only affords an explanation of the so-called "epidemic constitution" of certain years; but it would account equally for the alleged "pestilential type" of concurrent disorders, this type being present before the onset of an epidemic, and being itself a powerful predisposing cause of its spread.

In saying this, I do not wish to deny the possibility that certain conditions of the atmosphere may be more favourable than others to the low forms of life that we now know to be the active agents in zymotic disease, and the difficulty that we have found in accounting for the coincident variations of small-pox in distant towns might, perhaps, be solved by this hypothesis.

In these few remarks I do not venture to think that I have done more than open this difficult and obscure subject, and no one can be more conscious than I am of the doubtfulness of some of the explanations that I have advanced. I trust, however, that the Society will deem them worthy of criticism, and that they will agree with me in regarding the subject as one well worthy of further study.

OBSERVATIONS ON THE PRE-ERUPTIVE STAGE IN SMALL-POX, WITH HISTORY OF CASES.*

By MONTAGUE DUNCAN MAKUNA,
Late Medical Superintendent of the Fulham Small-Pox Hospital.

(*Read: April 5th*, 1882.)

ABSTRACT.

THE paper comprises observations on the pre-eruptive stage in small-pox in 626 cases, and the history of the pre-eruptive stage in 117 cases, forming the largest record of facts and *clinique* of the subject in medical literature. By pre-eruptive stage is meant the time which elapses between the date of the entrance of the poison into the system and the date of the first appearance of the eruption. Two reasons show why a distinct line of demarcation cannot be drawn between the period of incubation and the initial stage; firstly, no symptom is constant to usher in the initial stage; and, secondly, they are not definite in their duration. Dr. Richardson states that chills and rigors usher in the initial stage, but experience shows that this opinion is not altogether accurate.

Symptoms complained of by patients, and observed in 626 cases during the initial stage, were as follows in point of frequency. 1. Headache is probably the most constant. When it is localised, it is generally over the forehead. 2. Backache is most frequently confined to the lumbar region, and rarely extends above the dorsal. 3. Epigastric symptoms are: pain over the stomach, nausea, and vomiting; they are most frequent and severe in malignant cases. 4. Symptoms of lassitude, languor, prostration, and inability to walk about. 5. Symptoms of cold and sore-throat. 6. Pains over the body, and general malaise; constipation is one of the most frequent symptoms. Diarrhœa in an early stage, with high fever, cramps and rigors, is associated with intestinal eruption, as was determined in three cases. Other symptoms noticed in a few cases are faintings, stupor, delirium, pains over the præcordia, and syncope, constriction round the chest, epistaxis, photophobia and lachrymation, laryngitis, metrorrhagia, abortions, and typhoid symptoms. In a few cases erythema variolosa occurs, which is either

* Published *in extenso* in the *Med. Times and Gaz.* of May and June 1882.

macular, measly, scarlatiniform, or punctiform. In children we have fretting, grinding of the teeth, and convulsions frequently occurring.

The duration of the initial stage, or time of the primary fever, in these cases is given in the following table:—

TABLE *showing the Initial Stage in* 626 *Cases.*

Disease.	Total cases in each disease.	One day.	Two days.	Three days.	Four days.	Prolonged	None.
V. Varicelloides	22	6	6	5	1	1	3
V. Discreta...	316	64	115	52	34	39	12
V. Coherens	45	4	22	12	3	4	...
V. Confluens	203	42	67	48	11	29	6
V. Corymbosa	1	...	1	...	...	...	...
V. Maligna	39	7	13	11	1	6	1
Vaccinated	517	106	186	103	44	60	18
Unvaccinated	109	17	38	25	6	19	4
Total cases	626	123	224	128	50	79	22

It is most frequently two days; then follow three days, one day, four days, and so on. It is impossible to determine the causes of this variation, in the present state of our knowledge.

It will be observed from this table that the length of the duration of the initial stage bears no relation to the severity of the disease; nor has vaccination any influence over it. Of the 79 cases of prolonged duration it was five days in 19 cases, six days in 7 cases, a week in 24 cases, between a week and a fortnight in 17 cases, a fortnight in 10 cases, seventeen days in 1 case, and twenty-one days in 1 case.

The experience of those who have studied the various exanthems, leads to the opinion that the difference of the period of incubation in the same disease might be due (1) to some inherent qualities of the infective agent, be it vegetable or animal organisms, their germs, or, as has been recently stated by Professor von Nägeli, the products of organised bodies; (2), the difference may arise from the variations in the constitution of the victims and their surroundings, constitutions which are undermined by debility, previous maladies, bad habits, poverty, destitution, their concomitants, and the necessary evils arising therefrom, predisposing in no small degree to severe attacks of infectious diseases; (3), seasons of the year and meteorological conditions, inasmuch as they influence the nutrition of the body, govern also its physiological-pathological conditions; (4), constitutional in-

susceptibility to the disease, temporarily renders the action of the virus inert. That such variations do occur, is the experience of all careful and scientific observers, consequently the assumption that it is due to inaccuracies is purely gratuitous. Most dogmatic statements, laying down prognostications of the type of the disease from the duration of the period of incubation in small-pox, based on isolated observations, are found in the writings of authors, both at home and abroad. The assertions, remarks Dr. Murchison, are based on deficient observation. A perusal of the records of the cases would verify the statement.

Is the period of incubation infectious or not? It has been demonstrated that it is contagious. But there are no facts on record which prove that acute specific disease can be transmitted during the incubation stage, either by mere contact or through the atmosphere (Murchison). The history of 90 cases in the pre-eruptive stage of small-pox, positively proves that the period of incubation is non-infectious. The relationship of the cases to their primary sources of infection was as follows: 26 brothers, 18 sisters, 14 fellow-lodgers, 9 husbands, 7 daughters, 6 wives, 4 fathers, 3 sons, 2 mothers, and 1 brother-in-law. They all had lived in the same houses and occupied the same bedrooms respectively, with but few exceptions. The duration of the exposure to the source of infection was as follows: 2 for one day, 3 for two days, 8 for three days, 16 for four days, 12 for five days, 17 for six days, 4 for seven days, 1 for eight days, 3 for nine days, 1 for ten days, and in 23 cases it was indeterminate.

The duration of the pre-eruptive stage in 67 of these 90 cases was as follows: 8 days in one case, 10 days in three cases, 11 days in two, 12 days in six, 13 days in two, 14 days in seven, 15 days in seven, 16 days in thirteen, 17 days in eight, 18 days in six, 19 days in five, 20 days in three, 21 days in two, 22 days in one, and 23 days in one. The inter-eruptive period, or period which occurs between the first appearance of eruption in a primary case which is the source of infection, and that of the case or cases which originate from it, which closely corresponds to the period of incubation, was as follows in the remaining 23 cases: 13 days in two cases, 14 days in eight, 15 days in five, 16 days in five, 18 days in two, and 20 days in one.

It is evident from these cases that the period of incubation is non-infectious, for had it been otherwise, the disease would have recurred in the resulting or secondary cases, most of whom were bedmates, at a period very much shorter, or coincidentally with the primary cases.

Other cases demonstrate that the initial stage is infective. The period of incubation in 11 cases of single exposure was as follows: 6 days (with 7 days of initial stage) in one case, 8 days in one, 9 days in two, 11 days in one, 12 days in three, 13 days in two, 14 days in one.

It is stated that when susceptible individuals are exposed to the source of infection continuously, the period of incubation in them is much shorter. In 14 cases, it was as follows: 10 days in one, 11 days in three, 12 days in one, 13 days in three, 14 days in one, 15 days in one, 17 days in three, and 19 days in one.

The study of the period of incubation in acute infectious diseases teaches us their classification. That classification given by Dr. Squire, into two groups of those with short incubation periods and those with long, is the best. The practical importance of possessing a knowledge of the period of incubation is (1) for the determination of the time of quarantine; (2), for tracing the origin of an epidemic; (3), for the determination of the mode of introduction of the poison; (4), for showing when the vaccination and revaccination of persons exposed to infection during a small-pox epidemic will be attended with success; (5), it is stated that diseases of short incubation have prolonged convalescence and remain long as sources of infection; while diseases which have long incubation periods, have a quicker convalescence and more rapid freedom from danger as sources of infection; a knowledge of the length of the incubation period of infectious disease, therefore, is also important in this respect.

ON FILARIÆ AND OTHER PARASITES IN RELATION TO ENDEMICS AND EPIZOÖTICS.

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(*Read: December 7th*, 1881.)

THE nature of this paper is such as to require a few words of personal explanation. Having received several valuable manuscripts on the subject of Filariasis and other more or less nearly allied helminthic affections, and having been accorded every reasonable license at the hands of the respective writers, I have communicated the contents of these papers, either in whole or in part, to various societies and journals. I have thus incurred responsibility and no inconsiderable expenditure of time. However, if in the furtherance of the high aims of my correspondents, I have been compelled to postpone or altogether lay aside certain original inquiries, there has, at least, accrued to me the satisfaction of knowing that the labours of Manson, Bancroft, Sonsino, and others, were of far more importance in relation to public health than any results that could have been obtained by my own works. Moreover, the original investigations chiefly refer to comparative pathology, and the etiology of epizoöty in animals. It will better emphasise my meaning if I may say, that the only prolonged effort which a broken leisure permitted, has, at length, found expression in memoirs on the parasites of elephants and ostriches. The former paper will appear in the Linnean Society's *Transactions*; and if the facts therein set forth are duly weighed, I think it will be admitted that "flukes", are not the only entozoa capable of fatally victimising large and valuable animals.*

When, in April last, Dr. Myers's communication arrived, it was impossible to do justice to its contents. As it happens, no injury either to the merits of its industrious author or to the cause of science has resulted from the delay; unless indeed, the question of priority of verification be considered of particular moment. The facts on which Dr. Patrick Manson's beautiful discovery of the law of filarial periodicity

* Since published, *Linn. Trans.*, 2nd series, Zool., vol. ii, pt. iv, March 1882.

rests were originally communicated by me to the Quekett Microscopical Club, at the time that I was acting as its President. The first announcement was made on the 27th of February 1880. A second communication was read on the 25th of June of the same year; and the third and most complete memoir, was read on the evening of the 28th of January of the present year (1881). These communications were altogether independent of the three other papers of Dr. Manson, which I had read to the Linnean and London Medical Societies respectively. Here I may observe that no one took so great an interest in Manson's discoveries as Dr. Stephen Mackenzie, and it is a singular circumstance, that the first case in which it has been possible for a physician in this country to verify Manson's observations, should have occurred in the hospital practice of that gentleman. This is fortunate; for, in consequence of the observations made upon this case, some of the scepticism expressed in relation to filarial questions has already disappeared. Sooner or later it will surely be acknowledged, that these and other kindred discoveries, are second only in importance to those of Pasteur, Davaine, and others abroad, and of Lister, Sanderson, Carter, Roberts, and others at home, respecting the micrococci. I take leave to say, that Dr. Mackenzie's brief communication to the Pathological Society only too successfully concealed the large amount of observational work which he and his assistants accomplished. In this inquiry, the subordinate labours of Mr. Coates and Dr. Belfield deserve mention.

At the meeting just referred to, I suggested the recognition of five distinct epochs of filarial discovery. In doing so, my object was twofold; first, to point to the relative claims of separate investigators, and secondly, to correct what appeared to me to be one-sided and narrow views of the subject. How completely even the larger facts of discovery have been misread by some who venture to instruct others, is only too apparent from the leading article which subsequently appeared in the *Medical Press and Circular*; and how hopeless is the chance of witnessing any immediate general recognition of the *rôle* of the filariæ in the causation of disease, only those who have long worked at parasites can fully realise. If the objections raised [in respect of the power of the filariæ to produce the grave disorders with which they are commonly, but by no means invariably, associated] came only from those who have contributed little or nothing towards the advance of science, they might safely be passed over; but the misfortune is, that even eminent microscopists, and otherwise skilled observers, give encouragement to the scepticism which is so

prevalent on this subject. Especially sorry am I to find my views altogether out of harmony with the inferences drawn by so distinguished an investigator as Dr. Vandyke Carter. To be sure, the scepticism is not altogether confined to the *savans* of our own country, although, perhaps, it has here received a fuller expression. Scientific caution is one thing; but a prolonged resistance to conclusions, logically deducible from an overwhelming body of facts, is quite another matter. Even Professor Leuckart, who stands at the head of helminthology in Germany, did not at first accept the conclusions of Manson in reference to the mosquito as an intermediary bearer. Here is what he says, in a letter addressed to me from Leipsic, in the spring of last year:—"What you communicate concerning *Filaria Bancroftii* makes me very inquisitive, especially as I was hitherto inclined to regard the statements of Manson with some mistrust. At any rate he is, like Lewis, an helminthologically-schooled investigator."

In regard to the position taken up by Dr. Carter, I will at present only observe that if the filarial periodicity under normal conditions be only an "accident", as he alleges, then it follows that all other periodical nature-changes, whether similar or dissimilar, are merely accidental. The law regulating the succession of night and day falls within this category. The circumstance, so forcibly illustrated in Dr. Mackenzie's case, that by altering the conditions of the host, you can cause an inversion of the order of migration, points only to the immediate or probable direct cause of the phenomenon in question. That generalised expression of the facts, which we term "law", is not invalidated by the circumstance that an altered environment gives rise to a new phase in the operation of the law itself. Let the cause of the filarial migration be what it may, let it be due to sleep or rest, or aught else of change in the host's habits, its normal phenomena admirably fit in with the usual habits of the mosquito. The deception, artificially induced and practised on the filariæ by Dr. Mackenzie, if copied and maintained in filarial districts, would, for a while, inevitably deprive the female insects of their customary nocturnal worm-feedings, or, rather, worm-catchings; but, considering what Sir John Lubbock has taught us in respect of the capacity of ants to adapt themselves to novel conditions, artificially produced, it is tolerably certain that, in course of time, even the mosquitoes might, as it were, turn night into day. I think that the inverted order of migration obtained by Dr. Mackenzie tends to strengthen the view of Dr. Mortimer Granville, who supposes that sleep and the rest thereby induced are the im-

mediate cause of the migrations. They, at all events, appear to act as a summoner calling upon the filariæ to march.

Even the absence of power on the part of the mosquito to overcome habits that are probably the result of a long revolutionary process would not set aside the truth of our general proposition. If Dr. Carter really supposes that mere "accident" can explain migrations of a rhythmic character, such a conclusion rests, I think, upon a too limited view of migrational phenomena taken as a whole. Perhaps I am mistaken. Periodical journeys are short or long according to the necessities of any given species. In parasitic life, they are in constant operation, and probably their results are not less necessary for the welfare and propagation of these lower organisms than are the annual migratory flights of certain birds. Fifteen years back I offered a series of propositions respecting the *rôle* of parasites, and they embraced the statement that it is impossible for these creatures to continue in one stage, migration being essential to their prosperity, whilst any abrogation of this law could not fail to bring about their destruction.* At present, I see no reason to abandon any of the positions which a prior fifteen years of study of the entozoa had suggested.

It is well known that in Nature altered conditions will effect remarkable changes both in vegetable and animal life; yet few of us were prepared for the novel phenomena recently presented in the case seen at the London Hospital. At my request, Dr. Mackenzie has obligingly supplied me with the following recent particulars. Writing on the 10th ultimo, he says:—"A very remarkable change has taken place in my filarial patient since you saw him at the Pathological Society, October 18. At that time, we calculated he had from 29 to 37 millions of embryos in his blood at night (by counting the number in 10 *centimètres*, and calculating his quantity of blood as one-fifteenth of the body-weight). On the 20th, I gave him a holiday. It was a very cold and windy day, and he went out without his great coat. He came back in the evening, and in the same night had a rigor. His temperature went up to 104 deg. For some days, the pyrexia continuing high, it was uncertain what was taking place, but it eventuated in a pectoral abscess just in front of the left sterno-cleido articulation. Meanwhile, the filariæ completely disappeared from his blood, and the chyle from his urine after the 21st, and both have remained absent since. A point of some interest is that whereas filariæ mounted on October 19, travelled to Manchester and Surrey, and were quite lively

* *Entozoa: an Introduction, etc.*, 1864, p. 4.

the next day, filariæ on some slides put up on the 21st (the last night of their appearance) were dead on Saturday morning when they reached Dr. George Johnson. They evidently were sickly and short-lived. To return to the abscess. The possibility of the parent-worm being found there was not lost sight of; but when the abscess was opened, though we examined macroscopically and microscopically all the blood and pus that escaped, neither parent nor offspring were detected. The abscess has been dressed antiseptically, and the subsequent discharges carefully examined with negative results. The patient now has double pleurisy, but the temperature is falling and his condition improving."

So recently as Monday (December 5th), I have further learned from Dr. Mackenzie that "the embryo filariæ remain absent." He adds:—"Another abscess has been opened, but the parent-worm has not yet appeared. The urine is slightly opaque and mixed with blood."

If the full significance of the foregoing facts be not yet realisable, they at all events go far to illustrate, in an altogether unlooked-for manner, the truth of the proposition I have advanced respecting the consequences of any interference with the operation of the natural law of migration. If a nature-cure for the human victim be thus brought about, we can but rejoice in the fact, and strive to interpret the process. However, if in this case, the parent-worm or worms survive, I think we may look for a gradual return of the microfilariæ during the patient's convalescence.

In reference to Dr. Myers's hypothesis that there is a diurnal solution of the microfilariæ that escape the clutches of mosquitoes, I see nothing impossible in the disintegration and absorption of minute dead worms. On the other hand, I think that such an absorption-process could hardly overtake in a few hours the residual millions left by the mosquitoes. I believe that the microfilariæ, under ordinary circumstances, steadily increase in numbers, where no insects interfere, and that their maximum numerical expression is in strict correlation with the reproductive vigour of the parent-worm or worms. These may live for many years.*

I now proceed to draw more direct attention to the etiological significance of the filariæ in relation to endemics; but before doing so it is necessary to remark upon certain prevalent misconceptions. I do not refer to matters of mere inference, but to questions of absolute fact. In order to

* See the instructive case reported by Lewis, *On a Hæmatozoön*. Calcutta, 1874, p. 47.

combat these errors effectively I should have to give an exhaustive restatement of the successive observations, experiments, verifications, and deductions which have initiated and accompanied each of the five epochs of filarial discovery. That cannot be attempted; yet I will venture to affirm—obscure points to the contrary notwithstanding—that it is rare, indeed, that the method of biology receives so full an exposition of its scientific completeness as the record of these filarial investigations demonstrates.

The writer of the leading article in the journal already quoted, says, the "young filariæ develop in the mosquito into sexually mature worms." That statement is untrue. Throughout the whole range of helminthology nothing of the sort has been recorded. Whether we deal with *Cyclops* as the bearer of young guinea-worms, with *Gammarus* as the bearer of a trout's echinorhynchus larvæ, with *Melolontha* as the bearer of the hog's echinorhynchus-larva, with *Trichodectes* as the bearer of a canine tapeworm, with rats and mice as the bearers of a tapeworm of the cat, or with molluscs as the bearers of cercariæ, the result is invariably the same. Sexual maturity cannot be arrived at prior to the passage of the parasite into the body of the ultimate host. Again, the writer of the article in question states that "the free mature worm finds access to the bodies of human beings in a similar way to that by which *dracunculus* gains a hold on its victims". This statement is also at variance with ascertained fact. There is no such thing as a free mature *filaria sanguinis hominis*. Sexual maturity does not occur before access to the ultimate host is gained, but it follows and is consequent upon the introduction of the higher larvæ into the human body. Similar phenomena are witnessed in all parasites whose life-cycle requires a change of hosts. Again, certain passages of the writer do serious injustice to those who from first to last have been engaged in helminthological inquiries. I am not called upon to defend such an observer as Lewis, but the writer in question finds that in my recent "volume on Parasites" I was "unable to assert positively as to the genesis of *filaria sanguinis hominis*, holding, in contradiction to Dr. Lewis, that the worm is genetically related to *Bilharzia*". I certainly never made any such ridiculous statement, and I feel confident that Lewis can never have credited me with false teaching of this order. After such an exposition of the facts of filarial discovery as that referred to, it seems, to say the least, somewhat immodest on the part of the *Medical Press and Circular* to tell its readers, that on the occasion of the recent discussion at the Pathological Society, Dr. Vandyke

Carter and myself "succeeded in throwing little new light on the mystery still surrounding *filaria sanguinis hominis*."

In relation to the etiology of endemics it is of the highest moment that we recognise the power and also rightly interpret the mode of action of filariæ, hæmatozoa of all kinds, and microparasites generally, in the production of diseased states. The views which were long since advocated by me on this subject, though still unpopular, are beginning to be more generally accepted; and I should not be at all surprised if in some future leading article it was said of us:—"The helminthologists may be right, but they have taught us nothing new." At all events, without regard to the verdict of mere onlookers, I have hammered away at this subject in the hope of adding to the general progress of scientific medicine. The first element of scepticism in relation to the power of minute entozoa in the production of disease, rests upon the well-known fact that multitudes of these organisms may be present in man or animals without occasioning untoward symptoms of any kind. On this ground there are not wanting those who still deny that "rot" in sheep is due to the liver fluke, and they apply the same line of argument in connection with various epizoötics of parasitic origin. There is here a false interpretation. If the victim or bearer of the parasites be regarded, as he ought to be, as the legitimate home or territory of certain parasitic inhabitants, it will be found that in the case of minute entozoa the injury to the territory is primarily due to overcrowding. In a subsidiary manner the injury is due to the activity of locomotion of the parasitic inhabitants. A few thousands, or even millions, of trichinæ may bore their way through the tissues without occasioning the slightest inconvenience to the human bearer. In one hog that I experimented on we reared some sixteen millions of muscle-trichines, and yet the animal showed no sign of distress, nor did its appetite fail for a single day. Many millions of trichinæ are necessary for the production of grave symptoms of trichinosis in man; consequently, the presence of a few millions without the production of actual disease or suffering in man affords no proof whatever that the parasites lack the power of creating mischief. It is a question of massing. That which obtains in trichinosis obtains in the case of allied affections, such as filariasis, olulaniasis, strongylosis, cestode-tuberculosis, and so forth. It seems strange, indeed, that in the present state of our knowledge, the power of microscopic filariæ to cause disease should be doubted.

Nearly forty years have elapsed since Messrs. Gruby and

Delafond discovered filariæ in the blood of dogs. They examined 480 dogs, and they found microscopic worms in 5 per cent. of them. They did not settle the question of their origin. In 1867, I expressed the opinion that the microscopic filariæ were the young of *spiroptera sanguinolenta*. The researches of Lewis have since shown that this view was correct, as stated in my paper. Messrs. Gruby and Delafond "satisfied themselves that the hæmatozoa could only live in the blood itself, and they estimated that the verminous dogs severally entertained from 11,000 to about 224,000 of these larvæ. In no case, however, did the infested animals appear to have suffered inconvenience."* As regards the notion that the worms could only live in the blood, it is evident that the authors had no suspicion as to the migratory habits of the worms. To be sure, the law of filarial periodicity, does not assert itself in the dog as it does in man. To employ Manson's words, "there is a certain amount of periodicity though not so complete as in the case of *filaria sanguinis hominis*. Embryos are never entirely absent from the blood of the dog, though their numbers are always greater during the evening and night than during the day, the period of greatest scarcity being from about 9 A.M. to 1 P.M." In reference to the absence of suffering in dogs, Manson's experiences coincide with those of the French *savans*. He speaks of "vast numbers" of microscopic worms in the dog's circulation, and remarks that "one cannot but wonder that it is possible for a dog, containing such a prodigious number of parasites, to live". Infested dogs that have been under his observation for many years "seem in no way inconvenienced by their guests." On this subject I do not share the surprise felt by Manson. If instead of the presence of a few hundred thousand, there were tens of millions of the microscopic worms, then I should expect the canine territory to display some of the injurious results of overcrowding. Manson, of course, admits the terrible sufferings of dogs when the sexually mature, and therefore larger parasites gain access to the heart, and here it is easy to perceive that the immediate cause of pain, often followed by sudden death, results from the obstruction occasioned by the worms. In such cases the evils of overcrowding, though due to comparatively few parasitic inhabitants, are still equally manifest.

A second cause of scepticism in reference to the production

* "On the Comparative Prevalence of Different Forms of Entozoa Infesting the Dog, in Relation to Public Health."—*Journal of the Linnean Society*, 1867; *Zool. Sect.*, vol. ix, p. 291.

of disease by entozoa, is that which arises from a false interpretation of the fact of their almost constant occurrence in animals. Take the case of birds. As pointed out by me elsewhere, some think that tapeworms are not injurious to their avian hosts, because the parasites are nearly always present in greater or less numbers. Such persons forget that the tapeworm-fauna, as such, is a normal state of things, expressive of the distribution of the cestode species. Here again, excessive numbers produce epizoöty, and in the case of fledglings a comparatively small number of these worms will lead to fatal results. Where the epizoötics are due to smaller worms, young and old birds alike succumb to any undue expression of the fauna. Here it is, too, that we observe the operation of that law which Mr. Darwin has characterised as the struggle for existence. In my recent paper, read to the Linnean Society, I have shown that the South African ostrich-epidemic is due to a parasite which I have named *Strongylus Douglassii*. At least tens of thousands of these parasites occupy the proventriculus of young birds, and by the irritation thus set up, the chicks sooner or later succumb. It is only another instance of overcrowding. The same thing occurs in one form of grouse disease which I showed to be due to a nematode. This worm I called *Strongylus pergracilis*. In like manner, epidemics of husk in cattle and sheep are due to an abnormal development of the ordinary parasitic nematode fauna of ruminants, and there are several other ovine and bovine disorders strictly referable to the same cause. The so-called husk, lamb-disease, and worm-colic in animals, are only the professional terms of veterinarians signifying so many different forms of the special helminthiasis which I have collectively termed *strongylosis*. For the trichina-like disease of cats, I proposed the term *olulaniasis*. This has since been appropriated by one writer as if the term had originated with himself. This mild form of plagiarism is sufficiently common. It is high time that comparative pathologists should revise and scientifically classify the varied forms of epidemic disease due to parasites. As matters now stand, avian and mammalian epizoötics which are not due to parasites are apt to be mistaken for helminthic outbreaks; and, in like manner, the reverse may happen, parasitic equine epizoötics being apt to be mixed up with non-helminthic equine epidemics. Of course, the true nature of any epizoöty can only, in the first instance, be determined by investigators competent to recognise the different forms of parasites that may be present in particular outbreaks. The true nature of the Welsh outbreak, in which more than one

hundred ponies perished, would not have been determined had I not had an opportunity of verifying the facts on which the discovery rested. Its parasitic character, indeed, would probably not have been suspected had not special attention been previously called to the *rôle* of the parasites of our domestic animals by the publication of the only manual that then existed on the subject.

A third element of scepticism regarding the *rôle* of parasites in the production of disease, rests upon the assumed necessity for some pre-existing cachexia, or constitutional weakness on the part of the individual or animal attacked. This absurd hypothesis has been handed down from the oldest times, and is still often put forward as a novel conception on the part of its advocates. Probably, nine out of every ten medical practitioners hold this view. In the case of threadworms or *oxyurides*, this position is asserted with unbounded confidence, and I have been frequently told by parents that they were informed that their children would sooner or latter outgrow the disease. A pre-existing strumous diathesis is the favourite dogma taught, and thus the true cause of this widespread helminthiasis is either overlooked or ignored. It is within my professional experience to be able to assert, that the same dogma has been held to afford a sufficient explanation of the etiology of almost every known form of human helminthiasis. Views of this order were maintained by cultured physicians at the Rio Academy, during the discussion which followed the announcement of Wucherer's discovery respecting tropical anæmia or anchylostomosis, as it occurs in Brazil. The dogma of a pre-existing cachexia evidently still dominated the minds of the various speakers, modified a little perhaps to suit the exigencies of the occasion. As elsewhere stated by me (*Parasites*, p. 215), the general opinion was that the anchylostomes were not the primary and necessary cause of this tropical disease, but rather a co-operating agent. Against this view, it is gratifying to observe, that the amiable and enlightened Wucherer energetically protested. This I gather from the *Gazeta Medica* for January 1868; but his most complete memoir on the subject appeared in the *Deutsches Archiv für Klinische Medicin*, of September 1872. I regretted that I could only afford space for a brief abstract of it in my general treatise (*l.c., supra*, p. 213). Thus, it is not alone that the commoner parasitic disorders are regarded as entire or partial outcomes of some imaginery cachexia, but even the worst and most obvious types of tropical helminthiasis are set down to the same cause. Only the other day, a gentleman came to me suffering from the so-called Egyptian-

chlorosis. It is needless to say that this form of disorder (often called endemic hæmaturia), is, like anchylostomosis, generally associated with anæmia, or rather hypæmia, accompanied with more or less wasting of the tissues. Manifestly, the disorder is due to the loss of blood, consequent upon, and simultaneously passing away with, the eggs of *Bilharzia hæmatobia*. This gentleman was distinguished as an athlete; taking prizes in exhibitions of strength. Prior to the hæmaturia, not a trace of weakness marred his fine physique; nevertheless, although there was absolute proof of perfect freedom from struma, from phthisis, from syphilis, from dyspepsia, and, in short, from all forms of wasting disease, it was, I understood, surmised that there must have been a something in his system predisposing him to the parasitic disease from which he now suffers. Absurdity of inference, resulting from the domination of ancient ideas, can hardly go further. Not only are cachectic states not necessary to invite the invasion of parasites, but as a matter of fact they have not the smallest concern in the production of helminths. In cases of fungoid vegetable parasites, it is possible to admit that certain diseased conditions are favourable for the cultivation of algoid organisms; yet even here there are not wanting evidences to show that whole genera and species of fungi exist in perfectly healthly states of the host or bearer. Witness the so-called psorospermial sacs and their pseudo-navicellæ occurring in the healthy muscles of various animals; or, to go a little higher in the vegetable scale, notice the remarkable crops of genuine fungi described by Leidy as growing in the stomachs of certain American beetles (*Passalus*). Speaking of microphytes, Dr. Lewis says, "All the representatives of the groups of fission-fungi can be introduced into the system with the greatest impunity."

I must now turn very briefly to another phase of the question; and here I do not presume to teach others, but rather to solicit their help. Admitting, as a first point of departure, that the human filariasis (which shows itself under various forms of disease specially called elephantiasis, elephantoid fever, lymph scrotum, varix, helminthoma, chyluria, chylocele, and so forth) is due to overcrowding, it is physiologically important to determine how the various pathological results are more immediately brought about. It is also desirable to explain how the migratory movements to and from the circulation are accomplished. That such migrations do occur no longer admits of doubt, and that they are concerned in the production of injury to the host is a fact sooner or later destined to become generally accepted.

Guided by what Dr. Klein has said respecting the intimate connection between the vascular and lymphatic system, I had supposed that the young filariæ passed immediately, and by very short paths, from the smallest lymph-channels either into the capillaries or into the ultimate arterial branches. It is, indeed, far from proof that the migratory filariæ reside in the lymphatic system during the day. They may be resting within other tissues of the body, or as Manson suggests, within particular viscera. The concurrent abundance of filariæ, within the lymphatic glands and channels, has been proved by the actual tapping of groin glands in several cases under Manson's care. As regards the lymphatics and blood-vessels, I am informed by Dr. Hoggan, that this alleged close connection between the two systems does not really exist. In his view, the conclusions of Klein are based upon a false interpretation of appearances, observed in imperfectly or improperly injected preparations. Be that as it may, it seems to me that whatever anatomical arrangements obtain, the capacity of the worms to pass to and from the circulation in a direct manner, is in no way rendered doubtful. The borings of embryonic trichinæ, as well as those of many other microscopic worms, are perfectly independent of vascular arrangements, or the interposition of aponeurotic membranes. But what really strikes one with astonishment is the manifest power of selection which these parasites exhibit. Not only, under normal conditions, do they for the most part direct their course aright, but they also must have volition-power enough to recognise the tissues which form the walls of the particular vessel whose lumen they seek to reach. Without doubt, some of them stray from the right path, but such strayings are exceptional. The strayings observed to occur with other nematodes are often associated with the gravest disorders, leading not unfrequently to fatal results. In solipeds, as Böllinger has observed, "a whole host of morbid conditions" result from the wanderings of *strongylus armatus*. Some of these pathological states are brought about by what may be called the normal migrations, and others by the accidental strayings or abnormal migratory wanderings. On this subject the researches of Leuckart, together with the writings of Professors Böllinger and Friedberger, of the Munich Veterinary School, supply a vast amount of instructive evidence. In this country the experiences of Professors Simonds, Robertson, Axe, and others, point, I believe, in the same direction.

As regards the number and variety of disorders that result from mechanical obstructions by filariæ, so far from retract-

ing what has long been advanced on the subject, I am inclined to emphasise the correctness of our previous conclusions. Dr. Bancroft has stated that he could furnish a list of twenty diseases connected with *filaria sanguinis hominis*. He implies not merely accidental association, but that the filariæ are concerned in the production of these affections. I am myself far from thinking that all chylurias, chyloceles, and other phases of chylosis are exclusively due to parasites; nevertheless, all of these affections may directly arise from mechanical interruptions to the flow of the contents of the lymphatic channels. Thirty-five years ago I witnessed the drawing off of several quarts of milk-white fluid from the abdomen of a young girl; and, although the blood was not microscopically examined, it may be pretty confidently asserted that no filariæ were concerned in that chylosis. The case occurred at the Norfolk and Norwich Hospital, the tapping operation terminating fatally. To some it will at first appear that Bancroft's statement is exaggerated, but before it be denounced as such, due regard must be had to the views put forth by Lewis and Manson, and especially to the confirmatory and extended conclusion of Dr. Araujo, Dr. F. dos Santos, and other Brazilian physicians. The admirable summary of results, given by Dr. Bourel-Roncière in the *Archives de Médecine Navale* for 1878, has generally been accepted abroad as substantially correct. Throughout Brazil it is now customary to speak of all the disorders enumerated by Bourel-Roncière as so many separate phases of Wucherer's helminthiasis; and in the September issue of the *Gazeta Medica da Bahia* for the recent year, Dr. Pedro de Magalhães of Rio pronounces for the pathogenetic unity of hæmatochyluria, infiltrations, varices, fistulas, lymphatic abscesses, chylous ascites, elephantiasis arabum, craw-craw, and chylocele. For milky hydroceles, he suggests the term galactocele, previously employed by Vidal.

Drs. Magalhães and Araujo have contributed very largely to our knowledge of the filariæ and filariases as witnessed by them in Brazil; but, in addition to their contributions one must also particularise the labours of Drs. Silva Lima, Victarino Pereira, Julio de Moura, and likewise the recent exhaustive theses of Drs. Alfredo Luz and J. de Castro Rebello. In these essays, and also in Dr. Sousa's memoir on scrotal elephantiasis, the work of English physicians is closely followed up; the value of Sir Joseph Fayrer's contributions being fully recognised, especially in relation to Crevaux's corroboration of his statements. In this connection likewise, the observations of Drs. Paterson and Hall, more particularly

those having reference to the distribution and statistics of *filaria sanguinis hominis*, cannot be passed over. It may justly be said that these English residents at Bahia have largely helped to swell the mass of evidence produced by what may be called the "School of Wucherer". That which Drs. Bourel-Roncière, and Le Roy de Mericourt have so gracefully said respecting the researches of English physicians and helminthologists, we gladly accept as a bracketed verdict pointing to the value of the work done by our Brazilian contemporaries at Rio and Bahia.

OBSERVATIONS ON FILARIA SANGUINIS HOMINIS IN SOUTH FORMOSA.*

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ABSTRACT.

On taking charge of the Native Hospital at Takow in 1879, Dr. Myers began at once to examine cases of filaria disease, in order to investigate, and if possible confirm the discoveries and observations made by Dr. Manson of Amoy. His object was:—

1. To observe the proportion of filaria-infected patients in relation to the whole number of admissions to hospital, noting further what external manifestations (elephantiasis, lymph-scrotum, etc.) were present.

2. To note periods of appearance and disappearance of the filariæ from the blood of infected persons, with a view of corroborating or disproving Dr. Manson's discovery as to the periodicity displayed by the parasites in their appearance.

3. To account, if possible, for the disappearance of the filariæ at certain hours; discovering whether this was final as regarded the swarm, or whether they lay dormant and adherent, during certain periods, in the lungs or other organs of the body.

4. To attempt by experiments on monkeys to produce conditions similar to those observed in filaria-infected man.

In repeated examinations of the blood of patients admitted on the general list, at various hours both of day and night, he succeeded in finding only three filariated individuals. One of them was the subject of several observations, but the others refused to allow more than occasional examinations. From these and other conditions, detailed later on, he was led to think that this state of blood-infection was not common to, nor favoured by residence in Formosa, for the following reasons:—

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1. The number of patients and healthy persons who were fruitlessly examined both by day and night.

2. The almost total absence of those diseases which Dr. Manson has proved to be dependent on parasitic obstruction, such as elephantiasis and lymph-scrotum. Of the former, he had seen but one case, who informed him that she had contracted it "years ago in Amoy", and had been to hospital before for treatment.

The only statistics available for the native hospital date from 1871, and from that period up till April 1881, upwards of 15,000 patients had been seen, among whom only two cases of elephantiasis had been noted, and none of lymph-scrotum. From this fact alone, coupled with the discoveries of Dr. P. Manson as to the etiology of these diseases, he was strongly inclined to think that *filariæ sanguinis hominis* do not exist in the blood of natives of Formosa; otherwise many more cases of the above diseases would have been noticed during the nine years of which records exist.

On the mainland these diseases abound, whether one takes the earliest symptoms shown by the enlarged glands, the elephantoid parts, or the lymph-secreting scrotum. He is hence led to believe that the disease, when met with, is not of local origin, but has been imported from Amoy, or the neighbouring district, as was undoubtedly the case in the three instances he met with. A large proportion of the so-called "natives" or residents in Formosa are, comparatively speaking, recent colonists, coming originally from the Amoy district. Many of these, as well as their descendants, return, if only for a short stay, to the spot which family tradition styles their home; and even many of those who have become by length of residence apparently more closely attached to Formosa, seem, to some extent, to keep up this rule.

The Pep-hoans and Hak-kas, on the other hand, who have more right to be looked upon as regular and permanent natives of Formosa, and of whom a large number attend hospital, ought, one would think, if filaria-disease were common, to present some cases. This, however, was not the case, nor could Dr. Myers hear that elephantiasis or lymph-scrotum was met with, either amongst these people or the uncivilised aborigines. Had the conditions been favourable for further propagation, the cases introduced from time to time from the mainland would have given rise to such a prevalence of the disease as would have made its existence a matter of more certainty. The woman suffering from elephantiasis had, as she said, contracted the disease, when young, at Amoy, and though she, her children, and grandchildren had long been residents in Formosa, she and they

made periodical visits to the mainland, where several of their relatives still lived. She was a member of a large village, the residents of which belonged to the same clan, and though they had been a great many years in Formosa, and some of them born there, still they continued to consider themselves "Amoy men." Though she had no filariæ in her blood, *two were discovered in the sanguinolent lymph drawn from an enlarged inguinal gland.* She knew of no other cases of elephantiasis in her district.

Dr. Myers's inquiries in other parts of the country have led to a similar result, and though he heard of a case of elephantiasis scroti, he was told that in this case also the disease had begun during the man's residence on the mainland. His case appeared to be known far and wide, and to be spoken of with wonder, as if the disease was a rare one. Dr. Myers next gives an account of his attempts to filariate monkeys, as he thinks the observations made on filariæ after their ingestion by mosquitoes may suggest a reason for the non-propagation of the disease in Formosa. Dr. Manson, in his search for the intermediate host of the canine blood filariæ, found that the mosquitoes which nurtured the human parasites, when made to feed on an infected dog, digested the embryos they thus obtained, showing that the go-between must be of a different species in the dog's filaria to the human one. The patient was the boatman To-ah, a native of Amoy, whose history will be given subsequently. The plan followed was to make him sleep under a large gauze-covered cage ("mosquito house"), into which were put each night numbers of mosquitoes which had been freshly collected from all parts of the settlement, as he appeared equally indifferent both to their numbers and to their bites. A breeding-trough, also covered with gauze, was kept in the cage, and into it were put mosquito larvæ, got from different places, even Taiwanfoo, the capital city, thirty miles distant. As the mosquitoes developed, they were allowed to escape into the larger cage. A separate breeding-trough was kept in the darkest part of the house for those insects which had fed, to deposit their ova in.

Five monkeys, four of which were healthy, but one phthisical, were next obtained, and made to drink the water in which the mosquitoes had deposited their ova, and in which larvæ were already numerous. As these animals showed a strong repugnance to water containing any impurity or moving objects, they had to be drenched from time to time, during a period of more than six weeks. During the experiments, four of the five monkeys died of pulmonary disease; but neither in their blood during life, nor in the

survivor, could filariæ or any condition which was due to them, be detected, either during life or on *post-mortem* examination. On examining every day a certain number of mosquitoes which had been caught in the cage and preserved in carefully labelled bottles, Dr. Myers found numerous lively filariæ in those which had fed on the previous night; but at later dates he could find only semi-digested remains of parasites, which, later on, entirely disappeared. He was hence led to conclude that the Formosan mosquito was an unsuitable species for the intermediate stages of development of the human filariæ; and in this way he explains why no cases of elephantiasis or lymph-scrotum arise in Formosa, and how it was that the only filariated individuals he could get hold of were those hailing direct from the mainland. He intended, however, to make further experiments, if possible, with species of mosquitoes obtained from the mainland.

For the purpose of investigating subject No. 2, Dr. Myers was able to persuade only the boatman To-ah to submit himself for experiment, the other two patients proving refractory, and only with difficulty allowing even an occasional examination of their blood to be made, for purposes of corroboration. To-ah was under more or less supervision for six or eight months, during which several series of observations, agreeing in general results, were made. Only the most complete one is, however, detailed. The history he gave of himself in October 1879, is as follows:—

He is twenty-eight years old, and a native of Amoy, where he resided until he was twenty-one. From the time he was about fourteen or sixteen, he has suffered at various periods from "fever and ague". At about the age of eighteen or twenty he first noticed the swellings in his groin, which, however, have increased but little, in fact, he thinks they show a tendency to lessen in size. He suffers during the hot season from sharp attacks of "fever and ague"; otherwise he is in good health, well nourished and generally fit for his work. He was not aware he had filariæ in his blood, and does not think much of the fact, although he watched them under the microscope with much interest. He has visited Amoy twice since he first came to Formosa; but as his friends and relations have all died off, he thinks of permanently settling here. He does not suffer from any inconvenience whatever when pulling even long distances in the gig, nor does he find that he is unfit for considerable exertion of a pedestrian kind, and often accompanies his master shooting, carrying a tolerable weight all the time.

Is quite willing to allow me to make the experiments explained to him, and will be glad if he can be cured of his tendency to "ague" altogether, as "then he would be quite well." There is nothing abnormal to be seen about his scrotum or legs, and in every way he appears an athletic, well-developed man.

With reference to his attacks of so-called "fever and ague", Dr. Myers states that these are probably seizures of the characteristic ("lymphatic") fever, and differ from the malarial disease which they resemble, in the absence of marked periodicity, there being generally a long interval between each attack.

As to the other two cases which came under notice, they were admitted to hospital, the one suffering from ague, and the other from a callous ulcer of leg. No enlarged glands could be detected, nor any other ailment which might be attributed to filarial infection. The blood parasites were in both cases found in considerable numbers at night, and up to about 7.30 or 8 A.M. The blood was drawn from one or other of the fingers by slightly congesting them with a tape, and then pricking the skin with a sharp needle. *At least two slides were charged at each observation*—the maximum "find" being recorded. The Chinese assistant (whose observations Dr. Myers invariably checked), had been trained by Dr. Manson, and had, under his supervision and direction, assisted in similar work at Amoy.

Analysis of Series No. I.

It will be seen that the records amply bear out Dr. Manson's statements. The filariæ appeared regularly between 6 and 8 P.M., generally a little after six, as, in the fourteen observations, we find there were ten blank searches and four in which embryos were present. By 6.45 they had begun to appear regularly, although still in small numbers, and it was not until 7.15 that these had become considerable. By midnight they would seem to have attained their maximum, and from that hour gradual decrease set in. In the morning they would also appear to retire between the hours of six and eight, which gives a period of twelve hours during which they disport themselves.

As Dr. Manson has already pointed out, this is just the time when their liberators are in active search for food, proving one of those remarkable adjustments met with all through nature. Only on four occasions out of the fourteen was Dr. Myers able to see any filariæ in the interval from about 8.30 A.M., or 9.30 A.M., to 12 noon, though he diligently searched

for them, sometimes charging four or five slides. Every one of these appearances were put in at the noon examination, and on no occasion exceeded one.

The temperature was taken *before* drawing the blood, so as to obviate any risk of its being affected by the operation, if perchance this could be looked for. It will be noticed that at the hour when the filariæ return, the temperature rises slightly more than perhaps can be attributed to ordinary evening elevation.

Thus on thirteen occasions at 4 P.M., the thermometer showed 100 deg. once only; but on five occasions, it was 100 deg. at 6 P.M. On ten occasions, the temperature was 99·2 deg. to 99·4 deg. at 4 P.M., and five times, 99·3 deg. to 99·4 deg. at 6 P.M.

As soon as the filariæ had appeared in marked number (8 P.M.), the temperature did not go (save on one occasion No. 2) above 99·4 deg. and on three (Nos. 13, 14, 15), stood at 98·8 deg.; at 10 P.M. it again rose, however, as thrice the thermometer registered 100 deg., and at no time fell below 99·1 deg. At 12 mid., when the filariæ were in greatest number, the mercury appears to have fallen somewhat, as, though it registered 100 deg. once (No. 1), it was eight times at between 99 deg. and 99·5 deg., twice showing 99·1 deg. Four times it fell to 98·4 deg. to 98·9 deg. (Nos. 2, 6, 8, 12). By 4 A.M. there is a considerable fall, 98·2 deg. to 98·5 deg. being recorded eight times, with the filariæ in ample numbers. At 6 A.M. there is a slight tendency to rise, and from 8 A.M. onwards this is well marked.

On the whole therefore it would appear from this case that though the temperature bears no very marked relation to the numbers or activity of the filariæ, still some influence seems shadowed forth. Of course, with observations taken from one case, and only occasionally compared with two others, these temperature results are at present of little or no scientific weight.

Dr. Myers examined To-ah on several occasions during the time he was under observation, and found that his temperature was, as a rule, somewhat higher than normal, and tended to vary considerably in the twenty-four hours—this, too, at times when he had had no attack of "ague" for a considerable period and said he felt quite well. He therefore merely offers these temperature notes for what they are worth, trusting they may tempt some one, more advantageously placed for seeing a number of filariated persons, to take regular observations and prove or disprove what he can now only surmise as possible. In as far as the observations on filariæ, and the

periodicity of their advent or exit goes, this table may be more valuable, as it is an addition to observations taken by others, and in another locality, further borne out, as it is to some extent by the other cases, in as far as their complaisance would permit observations.

He next comes to the results of observations on what at the beginning of this paper was put as the third object for investigation, viz.: "To account, if possible, for the disappearance of the filariæ at certain hours; discovering, if feasible, whether this was final as regarded the swarm, or whether they lay dormant and adherent, during certain periods, in the lungs or other organs of the body."

In order to do this To-ah once more was induced to submit himself to examination for a series of seven days, commencing examination at 6.15 A.M., and taking observations at least every quarter of an hour until the filariæ had disappeared. On the following tables are given the results, with the usual record of simultaneous temperature.

From what Dr. Manson has written it appears that he is inclined to think that the embryos do not periodically dissolve in the blood, but that they probably congregate in some organ (possibly, the lungs), and there remain until the time arrives for their wanderings and withdrawal by mosquitos.

In favour of this Dr. Manson gives the results of *post-mortem* examinations of dogs, and, finding a great congregation of embryos in the lungs would—if analogy is thought to have any bearing on the matter—suggest that this supports the views he has advanced. Dr. Myers is not as much influenced by this as Dr. Manson would seem to be, for (as affecting the value of the experiment) he is inclined to lay considerable weight on the fact that the filariæ are never absent from the blood of infected dogs, but, on the contrary, are at all times circulating in considerable numbers. For this very reason, it appears to him that in a *post-mortem* examination, where the blood is in a state of stagnation, and a drop thus deliberately taken from any great hæmic collection, one would naturally expect to find the embryos thus secured in directly greater proportion to the mass of blood from which they are then drawn, than in the case of those escaping from a minute puncture in the smallest vessels, with the current rapidly sweeping past the opening.

If the filariæ be constantly watched after withdrawal from the body, the primary symptoms of debility visible will be the tendency of the animal to stretch itself out. At first, the oval, which it forms when in a state of vigour and activity, will be seen to become more and more perceptibly elongated;

the almost indistinguishable movements will appear more isolated; and as weakness increases, these change into a semi-undulatory action which at the last gradually ceases from the centre outwards, until the feeble motions of head or lash are all that remain to denote vitality. The lash becomes more distinct, and the integument puts on a somewhat shrivelled appearance. When death takes place, the embryo is seen to be extended, often at full length, and generally so to a great extent. The following are the notes made, on each occasion specified, of the conditions of the filariæ then seen. Days 1, 2, 3, and 4 were consecutive, but 5, 6, and 7 were taken with intervals between; and each morning and evening records were made on the same day.

DAY I.—6.15 A.M., 18 *Filariæ on slide.*—1st to 5th, normal; 6th, stretched out, languid; 7th to 10th, languid, stretched out; 10th to 14th, normal; 15th, weak, stretched out, body bent in distinct folds; 16th, very much stretched out, weak, only lower two-thirds of body appears to be moving; 18th, normal.

6.30 A.M., 12 *Filariæ.*—1st to 4th, normal; 5th, stretched out, decidedly weak; 6th and 7th, stretched out, somewhat languid, and pauses between contractions marked; 8th, stretched out, languid; 9th to 11th, normal; 12th, very weak, scarcely moving, shrivelled appearance.

6.45 A.M., 13 *Filariæ.*—1st to 7th, apparently normal; 8th, stretched out, languid, folds in body well marked and retained during appreciable pause; 9th, very weak, stretched round in circle, head only moving occasionally; 10th, brisker, but decidedly weak; 11th and 12th, normal; 13th, stretched out, lash distinct and moving vigorously, upper part of body much less vigorous.

7 A.M., 3 *Filariæ.*—1st, very weak, stretched out, what little motion there is undulatory; 2nd, more vigorous, but stretched out; 3rd, normal.

DAY II.—7.15 A.M., 5 *Filariæ on slide.*—1st and 2nd, extended, body movements not as vigorous as usual and more distinctly localised; 3rd, very slight motion at lower extremities, getting less and less under observation, body puckered and shrivelled; 4th, movement only in head, stretched out body, shrivelled appearance; 5th, body stretched to full length, slightest movement of lash, which ceased under observation.

7.30 A.M., 4 *Filariæ.*—1st, semi-convulsive jerks of body, the bends taking place at each movement being distinctly separated by inactive portions of body and apparently deeply indented, as though the body was more impressible, retaining the mark until the next movement took place, half stretched

out; 2nd, apparently as vigorous and well as usual; 3rd, movements much more sluggish, folds very slowly straightened out at each jerk, elongating itself, movements apparently involving same bends and folds of body every time; 4th, much more vigorous in action, but with marked tendency to elongation, and the pauses between motions well marked.

7.45 A.M., 4 *Filariæ*.—1st, very feeble in its action generally; 2nd, extended and markedly feeble, same symptoms of irregularity in action observed; 3rd, lively, and apparently as usual; 4th, very feeble, much stretched out, action spasmodic, with marked pauses.

DAY III.—6 A.M., 29 *Filariæ*.—Many lively, and apparently well, some very languid, scarcely moving, stretched out, with head and tail touching so as to form a large circle, apparently just dying.

6.15 A.M., 44 *Filariæ*.—Numbers of them were as usual, but others very debilitated, body stretched out and movements decidedly languid.

6.30 A.M., 9 *Filariæ*.—1st, languid, tendency to elongate and form circle by approximation of tail and head, folds and pauses marked, shrivelled; 2nd, very similar in condition; 3rd, still more feeble, folds in body very marked, giving an appearance as though body would break at each movement; 4th, stretched out and moving excessively languidly; 5th, ditto; 6th, quite dead. (N.B.—This is the first time I have come across a dead filaria in a drop of blood freshly drawn. The slide was, as usual, carefully washed and dried prior to use.)

6.45 A.M., 16 *Filariæ*.—1st, very languid, stretched out; 2nd and 3rd, quite stretched out, only very slight movement in head, both almost dead; 4th and 5th, ditto, ditto, creased appearance, very marked. The remainder were more or less debilitated and stretched out, with the exception of two which were quite vigorous and apparently well.

7 A.M., 9 *Filariæ*.—1st, lively and normal, though slightly inclined to stretch out; 2nd, stretched out in semicircular form, decidedly weak, movement more distinctly undulatory; 3rd, languid, stretched out; 4th and 5th, stretched out, somewhat weak in action; 6th, very feeble, movement slow and intermittent, made apparently with great effort; 7th, still less active, seemingly nearly dead; 8th and 9th, normal.

7.15 A.M., 26 *Filariæ on slide*.—1st, 2nd, 3rd, and 4th, nearly normal; 5th, stretched out, much more languid; 7th, stretched out, with undulatory movement; 8th, movements feeble, one extremity (oral) motionless, apparently paralysed, stretched out.

7.45 A.M., 5 *Filariæ*.—1st, feeble, stretched out; 2nd, ditto, very weak; 3rd, normal; 4th, slight motion, semi-paralysed appearance; 5th, normal.

8.15 A.M., 7 *Filariæ*.—1st, active, normal; 2nd, feeble, stretched out, undulatory movement; 3rd, feeble, stretched out; 4th, stretched out, activity not, however, affected; 5th, stretched out, rather feeble, and tendency to drag lower half of body; 6th, curled up, scarcely moving; 7th, normal.

8.30 A.M., 3 *Filariæ*.—1st, normal; 2nd, stretched out, movements feeble and intermittent, with distinct pauses; 3rd, normal.

8.45 A.M., 2 *Filariæ*.—1st, normal; 2nd, more stretched, movements less vigorous.

DAY IV.—6.15 A.M., 10 *Filariæ*.—1st, very languid and stretched round in wide oval shape, movements spasmodic; 2nd, very feeble, only moving after long pauses, and only at either extremity, the central portion of body being motionless; 3rd, barely moving, and then only with a weak undulatory motion, stretched out; 4th, stretched out, only movement in head, very spasmodic and irregular; 5th, normal; 6th, much extended, slow undulatory movement running spasmodically along the whole body beginning at tail, pauses very marked; 7th, quite extended, though movement is brisker and more regular than last; 8th and 9th, stretched out, slow feeble convulsive movement of head and tail; 10th, stretched out, very feeble.

6.30 A.M., 52 *Filariæ*.—1st, stretched out, action much more languid than normal, with appreciable pauses; 2nd, nearly normal, stretched out; 3rd, the same; 4th, curled up, very feeble; 5th, same as 3rd; 7th to 9th, stretched out, very weak; 10th to 12th, feeble, with spasmodic action exhibiting distinct pauses; 13th, stretched out, brisker than last, but pauses very marked; 14th, stretched out and very feeble; 15th and 16th, extended, feebly moving from head downwards in slow wavelike manner, getting feebler under observation; 17th, stretched out, motions spasmodic, with marked pauses; 18th, stretched out, otherwise normal; 19th, formed in wide ring, very feeble, movement of tail detected at long intervals; 20th, much brisker, stretched out; 21st and 22nd, nearly normal, stretched out; 23rd, motions spasmodic, slow, feeble; 24th and 25th, feeble, stretched out; 26th, much extended, very feeble; 27th, ditto, but more brisk; 28th, normal; 29th, 30th, 31st, very feeble, motion spasmodic and slow; 32nd, very feeble, stretched out; 33rd, the same; 34th, normal; 35th, normal; 36th to 40th, more languid than normal, stretched out; 40th to 45th, normal; 46th and 47th,

stretched out, very weak; 48th to 50th, stretched out, nearly normal; 51st, stretched out, very weak, motion spasmodic and slow, with long pauses; 52nd, normal.

6.45 A.M., 9 *Filariæ*.—1st, quite dead; 2nd, the same; 3rd, stretched out, very feeble; 4th to 9th, nearly normal.

7.0 A.M., 16 *Filariæ*.—1st, stretched out, nearly normal; 2nd to 4th, ditto; 5th, stretched out, very feeble; 6th, the same; 7th, normal; 8th to 11th, stretched out, feeble; 12th, stretched out, very feeble; 13th to 15th, ditto; 16th, stretched out, movement barely discernible, very feeble.

7.15 A.M., 31 *Filariæ*.—1st, stretched out, otherwise nearly normal; 2nd, stretched out, very feeble; 5th and 6th, stretched out and feeble; 7th, stretched out, very feeble, motion spasmodic and slow; 8th and 9th, stretched out, feeble; 10th, normal; 11th and 12th, stretched out, but motions lively; 13th, stretched out, motions slow and spasmodic; 14th and 15th, stretched out, motions slow and spasmodic, altogether very feeble; 16th, normal; 17th, very feeble, head only moving; 18th, stretched out, lower one-third of body motionless, giving paralysed appearance, movements of other portion spasmodic and slow, stopping suddenly at the quiescent part; 19th, stretched out full length, slow convulsive movements just discernible; 20th, stretched out, feeble, with slight spasmodic motion; 21st, stretched out, nearly normal; 22nd and 23rd, normal; 24th, stretched out, feeble; 25th, stretched out, very feeble; 26th, stretched out, feeble; 27th, normal; 28th, very feeble, formed in large ring; 29th, normal; 30th, stretched out, feeble; 31st, stretched out, very feeble.

7.30 A.M., 3 *Filariæ*.—1st and 2nd stretched out, very feeble; 3rd, nearly dead, stretched out, and motion when visible confined to lash.

7.45 A.M., 2 *Filariæ*.—Both very feeble, one much in same condition as No. 3 on last slide.

8.0 A.M., 6 *Filariæ*.—1st and 2nd, very feeble, scarcely moving, well extended; 3rd to 6th, very feeble, though less nearly moribund than first two seen.

8.15 A.M., 11 *Filariæ*.—1st to 4th, normal; 5th to 11th, all in different stages of debility, stretched out.

8.30 A.M., 20 *Filariæ*.—1st, normal; 2nd, stretched out, feeble; 3rd to 7th, normal; others more or less feeble (except 18th and 19th, which were normal), stretched out.

8.45 A.M., 6 *Filariæ*.—1st and 2nd, very feeble, barely moving, stretched out; 3rd to 6th, stretched out, feeble movements, though brisker than 1st and 2nd, still materially affected.

9.0 A.M., 3 *Filariæ*.—1st, very feeble, stretched out; 2nd, ditto; 3rd, ditto, nearly dead, movement only observed after long intervals.

DAY V.—7.0 A.M., 2 *Filariæ*.—1st, stretched out, motions slow, spasmodic and irregular; 2nd, very feeble, stretched out, folds or bends in body well marked and permanent, almost dead, movements only discernible at long intervals, body appears shrivelled.

7.15 A.M., 7 *Filariæ*.—1st, stretched out, feeble, movements irregular, with distinct pauses; 2nd, stretched out, semi-irregular spasmodic motions; 3rd, very feeble, stretched out; 4th, stretched round in wide circle, slightest movement visible, nearly dead; 5th, stretched out, slow undulatory movement at long intervals, very feeble; 6th, stretched out, very slight movement in head only; 7th, appears quite motionless and dead, but after long observation slightest movement detected in lash.

7.30 A.M., 1 *Filaria*.—Very feeble, only slightest movement in head.

7.45 A.M., 1 *Filaria*.—Stretched out, very feeble, slight spasmodic movement.

8.0 A.M., 1 *Filaria*.—Very feeble irregular movement at intervals, stretched out.

8.15 A.M., 1 *Filaria*.—Same as one on last slide.

8.30 A.M., 3 *Filariæ*.—1st, stretched out, very feeble; 2nd, stretched out, brisker than last, but still decidedly enfeebled; 3rd, stretched out, very feeble, movement irregular, spasmodic, folds or bends very marked.

DAY VI.—7.0 A.M. *No Filariæ*.

7.15 A.M., 1 *Filaria*, quite dead and shrivelled.

7.30 A.M., 4 *Filariæ*.—1st, very feeble, stretched out; 2nd, dead and shrivelled, lower one-third of body was very transparent and with difficulty made out; 3rd, stretched out, very feeble, body corrugated; 4th, stretched out, very feeble, slight movement chiefly confined to lash.

7.45 A.M. *No Filariæ*.

8.0 A.M., 1 *Filaria*.—Almost dead, stretched out, with flattened appearance.

DAY VII.—6.30 A.M., 11 *Filariæ*.—1st to 5th, stretched out, very feeble; 6th, apparently quite dead, extended in circular form; 7th and 8th, normal; 9th to 11th, stretched out and all more or less feeble.

6.45 A.M., 1st and 2nd, stretched out, very feeble, scarcely moving, and what motion there is confined to extremities.

7.15 A.M., 3 *Filariæ*.—1st, normal; 2nd, stretched out and much less vigorous, motion irregular and chiefly confined to

lower extremity; 3rd, coiled in two loops, very feeble irregular movement with marked pauses, evidently in last stage of debility.

The foregoing are the notes taken immediately on the slide being charged; and from the appearances therein described, Dr. Myers thinks it is evident that some decided change takes place in the filarial condition just prior to the retirement of the embryos from the circulation. The contrast with the vigorous, rapidly moving animal seen at night was very marked. *Then*, the movements are almost too rapid to be distinguished; *now*, in the majority of cases, irregular, and with distinct pauses. *Then*, the stretching out or extension of the body being but temporary, and apparently only with a view to alteration of position; *now*, is permanent and obviously the result of weakness.

It may be suggested that these are only the premonitory signs of approaching a temporary lethargy; and that those filariæ which seemed to be, and have been described as being, defunct, are not so in reality. He is inclined to doubt whether, if, in this state of stupor, any mere power of adhesion would enable them to resist the force of the blood current; indeed, it would appear more probable that, supposing they do congregate, during the hours of absence from the circulation, in any central part, much energy would be needed to maintain their hold; and, in fact, that in order to retain their position, it would be necessary for them to work at full speed against the tide, just as the typhoon-pressed vessel is sometimes able to preserve her place by "steaming to her anchors". Again, if torpor were the state indicated by the appearances described, the departure of the filariæ would be more simultaneous, instead of being so comparatively gradual.

As it is of great importance that extreme precautions be taken to avoid injuring the embryos in order to get reliable observations, it may not be out of place once more to allude to the consequences of carelessness in withdrawing the blood, or applying the covering slide. First, the blood drawn should flow or spring freely from the puncture; and this without any extraneous aid, save that afforded by the slightly constricting band previously applied to the finger. Should squeezing or rubbing of the part in order to force out more blood be attempted, Dr. Myers found that the serum was separated in the drop before it became large enough for transfer to the slide, and that the filariæ were invariably rendered feeble or languid in proportion to the distance they were removed from the corpuscles. Even on the slide,

towards the edges, it will sometimes be found that a band of serum is formed almost free from corpuscles, or with these in diminished number. Any embryos found here, or that may work themselves into these limits, are decidedly debilitated, or become rapidly so on arrival therein.

By watching such a slide, and the progress or accidental introduction of an embryo into the serous area, and contrasting its condition after deprivation of corpuscular contact with that previously observed, the effect will be more readily seen. For these and mechanical reasons, should the drop extracted be insufficiently copious to allow of the blood equally disseminating itself over the whole surface immediately covered by the upper glass, and pressure *other than the weight of the covering glass* be used to effect this, then the embryos will be weakened, and the preparation rendered unsuitable for the purposes in view. Of course, where the mere presence of the filariæ is all that is desired to be proved, these precautions are unnecessary; although if it be desired to ascertain the numerical relation of embryos to a certain, or as nearly similar quantity of blood as can be estimated short of actual measurement, then he thinks it will be found best to so make the puncture, that as far as one can judge, the same amount of blood springs from the orifice each time. He made the puncture in the middle of the second joint of any of the fingers with a No. 5 sewing needle, and *always felt the point against the bone.* Done rapidly, this does not appear to cause either increased pain at the time, or irritation afterwards; and for the constriction of the finger, he found a divided india-rubber letter-band bound lightly round the first joint quite sufficient. Knowing these risks, and with them fully before his mind, he observed the greatest care in preparing the slides which formed the subjects of his investigation into the relative condition of the filariæ extracted in the morning and evening, and can only say that if error has crept in, he has done what was possible to avoid it; but at the same time, where so much depends on one's ability to appreciate and estimate symptoms almost entirely consisting of comparative degrees of motion, a single observer may easily be misled or mistaken.

In order to test the longevity of the filariæ after withdrawal from the body, slides were prepared with all precautions at 9.30 P.M., and from 7.45 A.M. With a view of preventing desiccation, the slides were carefully oiled for a space of about a quarter of an inch round their rims. Blood was then drawn in the manner before described, the covering glass (of similar size and thickness to that on which the

blood lay) was then carefully adjusted, and only those preparations were selected on which the blood and its corpuscles were, as nearly as could be judged, equally disseminated. In order to ensure greater opportunities for contrast, blood was extracted at 7.45 A.M. and 8.0 A.M., these being the morning times which experience seemed to suggest as being the most suitable. Each embryo was carefully observed, and its condition noted, and the following results were obtained. The notes are arranged in parallel columns for facility of comparison.

Morning Slides.	Evening Slides.
1st Day.	*1st Day.*
1st observation, 7.45 A.M.—12 *Filariæ on slide.*	9.30 P.M.—31 *Filariæ on slide.*
1st F., stretched out; spasmodic action; extremities more active than centre; very languid.	Every one appeared to be vigorous and moving with great rapidity, presenting a "star-like appearance"; motion so rapid as to be quite undistinguishable; no stretching out or other sign of debility to be seen in any of the embryos coming under observation.
2nd F., stretched out; extremities active; generally languid.	
3rd F., stretched out; almost dead; extremities slightly moving, with slow undulatory motion.	
4th F., stretched out; extremities very active; motion spasmodic.	
5th F., stretched out; general movement but less vigorous than normal; action spasmodic, with distinct pauses.	
6th F., stretched out; very weak; action spasmodic.	
7th F., more active; stretched out; pauses very marked.	
8th F., very languid; two-thirds of body gyrating slowly round; passive upper third, which is extended.	
9th F., very nearly normal in action; slightly stretched out.	
10th F., very languid; about two-thirds of body moving spasmodically; stretched out.	
11th F., stretched out; spasmodic, interrupted action; quite languid; remaining passive for some time.	
12th F., stretched out; less languid than last, though evidently weak.	
8 A.M.—7 *Filariæ on slide. 1st observation.*	
1st F., stretched out; very languid; one portion gyrating slowly round passive part.	
2nd F., languid, gyrating spasmodic action; stretched out.	
3rd F., stretched out; very languid.	

MORNING SLIDES.

4th F., stretched out in long semi-oval; very feeble.
5th F., stretched out; spasmodic action; very languid; motion confined to extremities.
6th F., more active than last; stretched out.
7th F., stretched out; nearly normal action, though pauses are discernible.

1st Day.

2nd observation. — Preparation 12 hours old.—7.45 A.M. *slide,* only 8 *Filariæ* now visible.

1st F., quite dead.
2nd F., stretched out; languidly moving.
3rd F., much the same as last.
4th F., ditto.
5th F., more vigorous than others.
6th F., stretched out, but very vigorous.
7th F., stretched out; languid.
8th F., stretched out; much more feeble; what action there is, spasmodic.

8 A.M. *slide, only* 5 *Filariæ now visible.*

1st F., upper two-thirds of body curled and slowly moving on lower one-third; very feeble.
2nd F., ditto.
3rd F., stretched out; weak undulating motion.
4th F., stretched out; more vigorous.
5th F., stretched out; very weak.

2nd Day.

3rd observation at 9 30 P.M.—*Preparation* 36 *hours old.*—7.45 A.M. *slide,* 8 *Filariæ only visible.*

1st and 2nd F., dead.
3rd F., wriggling itself in knots; then passive for two or three seconds, convulsively freeing itself; pausing a like time, and then repeating the process.
4th F., stretched out; very attenuated; resting by head and tail very feebly; swaying rest of body.
5th F., stretched out; more vigorous; spasmodic action with long pauses.

EVENING SLIDES.

2nd Day.

2nd observation at 9.30 P.M.—*Preparation* 24 *hours old.* —9.30 P.M. *slide,* 31 *Filariæ visible.*

All more stretched out than on previous evening, and the movements were less vigorous, especially about oral extremity. Two filariæ seemed less active than rest, but otherwise no very marked signs of debility.

Morning Slides.	Evening Slides.
6th F., head moving languidly to-and-fro; lower part of body convulsively starting occasionally, but no general movement; very feeble. 7th F., moving languidly with undulatory motion along body; distinct pauses; stretched out to full length. 8th F., quite dead; attenuated, transparent, and shrivelled up.	
8 A.M. *slide, 36 hours old, 5 Filariæ visible.* 1st and 2nd F., dead. 3rd F., very feeble; motion in oral extremity, otherwise passive. 4th F., ditto. 5th F., very languid movement in both extremities.	
3rd Day.	*3rd Day.*
4th observation at 9.30 P.M.—Preparation 60 hours old.—7.45 A.M. slide, 5 Filariæ visible.	*3rd observation at 9.30 P.M.—Preparation 48 hours old.—9.30 P.M. slide, 31 Filariæ present.*
1st F., only upper third of body present; this very transparent and attenuated. 2nd F., Lower one-third gyrating slowly and convulsively round rest of body, which is perfectly passive. 3rd F., similar appearance, though the active portion is more vigorous. 4th F., stretched out; very feeble; undulating motion visible at long intervals. 5th F., stretched out; slightly more vigorous. No traces of others on slide.	All more or less stretched out, and motion more undulatory; lash, in one or two instances, becoming visible; one very feeble, though movement was general as to the body.
4th Day.	*4th Day.*
5th observation at 9.30 P.M.—Preparation 84 hours old.—7.45 A.M. slide, 2 Filariæ visible.	*4th observation at 9.30 P.M.—Preparation 72 hours old.—9.30 P.M. slide, 30 Filariæ visible.*
1st F., very nearly dead; motion very feeble. 2nd F., quite dead. No others, nor traces of others, visible on slide.	All more languid than last night, but still movements in most cases very lively. Three filariæ appeared specially weak, and central portion of body nearly passive; but the general similarity of condition, taken as a whole, was very striking.
8 A.M. *slide, 2 Filariæ visible.* 1st F., very feebly moving. 2nd F., only about two-thirds of	

MORNING SLIDES.

body present; shrivelled up and attenuated.
No others, nor traces of others, to be seen.

5th Day.

6th observation at 9.30 P.M.—*Preparation* 96 *hours old.*—7.45 A.M. *slide,* 1 *Filaria visible.*

After long watching, the oral extremity is seen to move in fairly perceptible manner.
No others, nor traces of others, to be seen.

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12 hours afterwards both slides examined and no traces of filariæ could be observed on either.
The examination of each slide took about 40 minutes, and they were closely inspected several times on each occasion.

EVENING SLIDES.

5th Day.

5th observation at 9.30 P.M.—*Preparation* 96 *hours old.*—9.30 P.M. *slide,* 25 *Filariæ visible.*

General languor marked, and motion more generally confined to extremities.
14th and 18th F. were very weak, and had to be watched some time before motion other than in oral extremity could be detected.

6th Day.

6th observation at 9.30 P.M.—*Preparation* 120 *hours old.*—9.30 P.M. *slide,* 20 *Filariæ visible.*

12 F. much stretched out; undulatory motion running along body; pauses distinct.
13th to 16th F., much more vigorous.
17th and 18th, quite dead; attenuated.
19th F.; movement like the first, embryos observed.
20th F., only two-thirds of body present; very transparent.
No traces of others visible.

7th Day.

7th observation at 9.30 P.M.—*Preparation* 144 *hours old.*—9.30 P.M. *slide,* 12 *Filariæ visible.*

1st to 8th F., movement decidedly feeble and spasmodic.
9th to 12th F., quite dead; transparent. Two-thirds of another embryo was visible; it was very attenuated. No traces of others visible.

Morning Slides.	Evening Slides.
	8th Day.
	8th observation at 9.30 P.M.—*Preparation* 168 *hours old.*—9.30 P.M. *slide, 4 Filariæ visible.*
	1st F., very feeble motion of both extremities; central part of body passive.
	2nd F., quite dead.
	3rd F., very languid movement in lower extremity, which had to be watched for some time before it was detected.
	4th F., quite dead.
	There were traces of two or three others, or what appeared to be portions of others.
	An examination made twelve hours afterwards failed to detect any living embryos, and only one dead one was seen, with traces of two others.
	The slide was examined for about forty minutes each day.

It will thus be seen that twelve hours after extraction, out of the 12 Filariæ originally contained on the 7.45 A.M. slide, four had disappeared; and on the occasion of making the second observations there was one dead embryo on the field. With 8 A.M. slide, after twelve hours two had disappeared; twenty-four hours after this, that is, thirty-six hours after withdrawal, three dead filariæ were visible; but there was no diminution in the number of embryos on the 7.45 A.M. slide. With the 8 A.M. slide the numbers were the same as at last examination; but now two of the embryos were seen to be dead. In sixty hours only five embryos, or rather $4\frac{1}{3}$, were to be seen on the 7.45 A.M. slide, making a total loss in the time of $7\frac{2}{3}$. The 8.0 A.M. slide does not appear to have been examined at this time, for there is no note of it; but in eighty-four hours, only two embryos were visible on the 7.45 A.M. slide, one of which was quite dead, making a loss in this time of 10 filariæ. On the 8.0 A.M. slide, there were also only two embryos visible, or rather $1\frac{2}{3}$; and in ninety-eight hours but one filaria, *in articulo mortis*, was to be seen on the 7.45 A.M. slide.

With the evening slide, Dr. Myers found that up to twenty-four hours the numbers remained undiminished, and the condition satisfactory; though certainly less vigour was displayed than at the first observation, and it was not until

forty-eight hours after withdrawal that the first embryo disappeared, and very decided signs of debility were present. In seventy-two hours only two embryos had disappeared; but with the exception of these the general condition was remarkably good. In ninety-six hours, languor became very marked; but still only five embryos had disappeared, and in 120 hours, ten had vanished, and the dead and dying were met with for the first time in the field. In 144 hours, but twelve embryos remained, and again the dead filariæ were to be seen. At the eighth observation, that is, 168 hours after withdrawal, only four embryos were to be detected on the slide, two of which were quite dead, and the others nearly so. Twelve hours afterwards, all had disappeared. In a word, the embryos withdrawn in the morning, and treated in exactly similar manner to those withdrawn in the evening, *were all more or less weak when first extracted, and had all died or disappeared two or three hours after the termination of the fourth day*, whereas those withdrawn in the evening had not all disappeared or died, until an hour or two past the seventh, besides being very much more vigorous at the outset, and preserving their vivacity for a considerably longer period.

It may be asked why, if the embryos die every twenty-four hours in the host, do they live so much longer when liberated? In reply to this, Dr. Myers suggests that up to a certain time the act of withdrawal is a compliance with the natural requirements of the parasite, and that whereas it is not impossible that continuance in the circulation of the host, beyond the time allotted to them, may set up influences designedly calculated to bring about a rapid fatality, and thus clear the way for the new swarm, on the other hand, some time might be necessary before the inadequacies of the artificial state made themselves felt, and thus in lieu of sudden and general destruction, we have the gradual decadence of strength and vigour exhibited by the embryos preserved on the slides. As to the cause of disappearance, he does not feel sufficiently informed to venture more than a surmise. Every precaution which care and deliberation could afford, was taken to avoid missing embryos in various searches, and he thinks the condition of portions of filariæ seen, and the appearance of these, tends to lend weight to the supposition that *solution* may also be the final process here by which removal is effected; at least it would appear from what was seen, that this was the most reasonable and probable method of accounting for what is, no doubt, a very remarkable and puzzling phenomenon. It may be that all

he has observed is peculiar to the solitary case on which he was able to experiment, and it may be found that with different subjects different results are obtained. This, time and future investigation alone can determine.

As some experiments made by applying various matters to the blood, in order to observe the effect on the filariæ, may be of interest, apart from the immediate object for which they were undertaken, Dr. Myers described them briefly.

To the preparation a drop of water was added, and from this the most speedy effects were visible. The blood corpuscles were washed away from the animals, and the latter absorbing the fluid became dilated, enfeebled, and rapidly killed. The great anxiety of the filaria seems to be to get in contact with the red corpuscles, and as soon as it becomes affected this anxiety is more marked. To another preparation were added a few fine crystals of arsenious acid. The filariæ immediately began to stretch out, both they and the corpuscles becoming very transparent. The animals moved in a feeble, jerky manner, and in 38 minutes, the first death was noted. It was astonishing, however, what a comparatively large quantity of the drug it took to bring about this result. To a third slide was added salicylic acid, when again the extension and enfeeblement began immediately, and gradually increased until eight hours afterwards, when the first dead filaria was seen.

To a fourth drop santonine was applied, and although the embryos immediately began to extend their bodies, and slow symptoms of debility, the effect of this medicine was very much less marked than that of either arsenic or salicylic acid.

Quinine (the bi-sulphate was used on account of its solubility) had a rapid effect in reducing the animals to the last stages of weakness, indeed, apparently a more speedy one in this respect than the arsenic, but Dr. Myers was not able to be sure of the death of an embryo until five or six hours had elapsed. When To-ah was taking quinine (and so it was with the other two patients), it was always noticed that the filariæ were much less lively and healthy-looking, and for this reason Dr. Myers was particular in seeing that he had taken none for some time before he made the observations set forth.

As the question, however, is not how to destroy the blood embryos, but rather how to get rid of their parental source, these therapeutical observations do not point to much that could not have been previously surmised, save that it seems

likely that before an effective result could be obtained from the use of these drugs, the blood would need to be so saturated that the remedies would probably act towards both host and parasite in a manner, the impartiality of which would defeat the end desired.

It now seems tolerably certain that the *locale* of the mature worm is in the lymphatic system, and generally in the more superficial glands, so that the exact habits of the parasite and the situations most frequently selected being first ascertained, it may be found that help lies more in the surgeon's knife than the physician's medicaments, though of course, with so indefinite a range, it is also highly probable that more than partial relief, in as far as the presence of embryos in the blood is concerned, cannot be very sanguinely anticipated.

With dogs, though the worm is found in other parts of the vascular system, still, the greatest number abide in the heart, and undoubtedly from that position effect all their mischief: in man, the home of the parasite appears to be less desperately localised; and if it should happen that the favoured and most important lodging is one accessible from the surface, the human sufferer will not only have much of the despair alleviated which observations on the canine subjects might tend to set up, but he may hope that those parasites which have taken up their abode in other and deeper situations may, like the extra-cardiac animals in the dog, continue their existence without imperilling or materially inconveniencing that life on which their own depends.

Unfortunately, in China, where *post-mortems* are so strictly prohibited, much progress in solving the remedial problem cannot be hoped for: but, perchance, clues obtained here from the living, may lead to satisfactory results in lands where the pathologist is looked on with less horror and detestation than he excites in that country.

ON FILARIA SANGUINIS HOMINIS, LYMPHOCELE, LYMPHURIA, AND OTHER ASSOCIATED MORBID DISORDERS.*

By PROSPERO SONSINO, M.D., Pisa.

(*Read: May 3rd*, 1882.)

ABSTRACT.

THOUGH Dr. Sonsino first detected the embryo filaria in Egypt in 1874, he has not yet had an opportunity of personally verifying Bancroft's discovery of the adult worm. Having since his first publication in 1876 had several cases of filaria disease under his care, he gives the following tabular statement of them. (See next page.)

(1.) The first case observed was interesting as being subject to hæmaturia due to bilharzia, as well as the host of filariæ, which appeared to give him but slight trouble. During the eight years he was under observation, the deafness and slight febrile attacks from which he suffered have improved; and though filariæ are still present in his blood, he has never suffered from lymphuria, or elephantiasis.

In the second case, the parasite was associated with elephantiasis of the scrotum and prepuce, with abscesses in the legs, from which some specimens of dracunculus medinensis had been extracted. He finally succumbed to general tuberculosis, but no adult worm was found at the autopsy.

In the third and fourth cases, lymphuria was met with, causing in the former no obvious damage to the patient, while in the latter case there was great emaciation.

The fifth and sixth were cases of lymphocele, or milky hydrocele, and in the fluid obtained by tapping, as well as in the blood, filariæ were abundant.

The seventh, eighth and ninth cases, were instances of severe lymphuria, causing great emaciation and debility, while the tenth, like the first, came under treatment for severe hæmaturia due to bilharzia in 1874, and though now in good health and free from the latter parasite, his blood still presents numerous filariæ.

He gives the following description of the urinary troubles which were met with in five out of the ten cases:

* Published in the *Med. Times and Gazette*, May 1882.

Synopsis of Ten Cases of Individuals affected by Filaria Sanguinis observed in Egypt.

	Years of the observation.	Where observed.	Origin, profession, state, etc.	Age (years).	Disorders and diseases associated.	Where embryonal filariæ were found.
1	1874-82	Private practice, Cairo	Native Jew, merchant	15	Deafness and attacks of ephemeral fever, bilharzia disease, ascaris, and oxyuris	In blood from the finger, and once in the bloody urine.
2	1877	Kasr-el-ain Hospital, Cairo	Negro Bongo, soldier	22	Elephantiasis scrotalis and præputialis, abscesses in the thigh, marasmus, and diarrhœa ...	In blood taken from the scrotum.
3	1877	Private practice, Cairo	Native Jewess ...	55	Lymphuria	In blood from the finger and in the lymphous urine.
4	1880	Deaconess Hospital, Alexandria ...	Native Copt, clerk ...	50	Lymphuria and emaciation ...	In blood from the finger and in the lymphous urine.
5	1880	Greek Hospital, Alexandria	Greek, since many years in Egypt, coffee-house servant ...	26	Lymphocele	In blood from the finger and in the fluid of lymphocele.
6	1880	Deaconess Hospital, Alexandria ...	Mohammedan from Gabes (Tunis), coffee-house servant ...	20	Lymphocele	In blood from the finger and in the fluid of lymphocele.
7	1880	Private practice, Cairo	Native Caraite Jew, merchant	30	Lymphuria, with anæmia and weakness	In blood from the finger.
8	1880	Private practice, Cairo	Native Jew, clerk ...	30	Lymphuria, with anæmia and weakness	In blood from the finger and in lymphous urine.
9	1881-82	Private practice, Cairo	Native Mohammedan, apothecary	23	Lymphuria, emaciation, chronic pneumonia, partial muscular disorders	In blood from the finger and in lymphous urine.
10	1882	Private practice, Cairo	Native Jew	23	Bilharzia disease	In blood from the finger; once in bloody urine, in 1874.

Symptoms and Characters of Lymphous Urine.—The attack of lymphuria (called commonly chyluria) comes on generally suddenly, while the patient seems in full health. It begins with pain in the loins, and very often with ischuria. After some delay, the micturition is performed with great difficulty, and urine is voided presenting the strange character of having the appearance of milk. Sometimes, also, the white or yellowish-white urine has some reddish or pinkish streaks, due to blood. Occasionally, either at first or in the course of the attack, it is entirely bloody. At times it is accompanied by some fibrinous or bloody urethral casts, which the patients think are pieces of flesh. The difficulty of micturition is due to the coagulation of the lymph in the bladder, and it is only when the coagula begin after some hours to be dissolved that micturition becomes possible. At other times, coagulation in the urine takes place only after emission, when the clot takes the form of the containing vessel, and is a tremulous, elastic jelly, which may constitute two-thirds or three-quarters of the total urine. This coagulum becomes afterwards firmer, a portion of the fluid being squeezed out by retraction of fibrin; but after some hours the coagulum is completely dissolved, perhaps through the urine becoming alkaline. The urine voided in the course of the day often offers a brown hue, like milk and coffee, which is due to altered blood, whilst in the morning hours it is more like simple milk. The urine sometimes is acid, sometimes alkaline, more often neutral; specific gravity variable, generally from 1012 to 1020. Urine with the above characters may be passed for weeks and months. The lumbar pain often subsides, reappearing occasionally. Generally, patients feel weakened by this drainage of lymph, and after some time become thin and emaciated; but in some cases, as in the third one given, the loss of lymph is well borne. This might be attributed to good hygienic conditions of the patient—in fact, the third case is that of a lady who can afford to live with every comfort.

In the course of an attack of lymphuria there are often remissions in which the urine may present a normal appearance, and when opaque, may have only a very little coagulum. At last the urine acquires permanently its normal characters and the disorder is in abeyance for a shorter or longer period.

Lymphuria is generally only an intermittent disorder, with attacks of different duration, and with interruptions of months, and even of years. But the attacks generally return in time. Of the five cases observed, the attack only once lasted so long as thirty-two months.

Filariæ in the Lymphous Urine.—In the urine of the five

patients he constantly found embryonal filariæ in more or less quantity. It is especially in the clots or coagula that these embryos can be discovered with more or less ease. Indeed it seems that the fibrin when coagulating takes up in its meshes all the embryos, as it is very rare that we find any in the liquid after coagulation. Generally, even if the examination is made soon after emission of the urine, the embryos appear dead; or, if living, their movements are very slow. This is very different from that which he has verified in regard to filariæ in the fluid of lymphocele. He supposes that the ordinary acid urine is favourable to their vitality. Dead embryos present a granulated appearance in the axis, that is not apparent in the living filaria in its full activity. In lymphous urine he found filariæ at all hours when the urine was emitted. To recognise filariæ in lymphous urine is not so easy as in the blood, generally because they are dead, and also because the fibrillar appearance of fibrin coagulated may lead to mistake, which, however, is not possible with due care and the use of an appropriate focus.

Microscopical and Chemical Characters of the Lymphous Urine.—With the microscopical examination the fibrillar appearance due to fibrin is found, besides white or lymphatic corpuscles, some epithelial cells of the urinary tract, and more or less red corpuscles altered by urine, as well as a great quantity of very minute granulations of different appearance, which probably are constituted both by proteic and fat matter. The fluid left by the spontaneous coagulum is coagulable both by heat and by nitric acid. The coagulum by nitric acid has reached sometimes one-third or one-half of the total of the liquid. When lymph is not in large proportion, and the urine is alkaline, heat alone may not yield any coagulum, and to obtain it, it is necessary to add a few drops of acetic acid. The fluid urine mixed with ether becomes presently more transparent, and with time it appears divided into two strata, one offered by the ether that has dissolved fat, and the other by a coagulum of albuminous matter. With chloroform, after some hours we have four strata—the superior one transparent, constituted by pure urine; then a second stratum yellow, like cream (fat); a third constituted by a little brownish coagulum; then the fourth, the more abundant, and quite white, constituted by coagulated albumen. Therefore there is no doubt that lymphous urine contains fibrin, demonstrated by the spontaneous coagulation, albumen in great quantity, and fatty matter. This latter is certified by the smell of acrolein after evaporating the ether and burning its residue. For this chemical examination he is indebted to Mr. Sickemberger, an apothecary of Alexandria.

A Case of Lymphuria without Filarial Embryos.—There is no doubt, he thinks, that lymphuria, as well as lymphocele, is originated in Egypt by Filaria sanguinis, yet he observed a most characteristic case of lymphuria in which he could not find filarial embryos, either in the blood taken from the finger, or in the lymphous urine. The patient was a native mulatto, aged thirty-nine, a shoemaker, whom Dr. Sonsino first visited in September 1874, some time after he had been taken by a sudden and severe attack of lymphuria, which at the beginning caused some danger from ischuria. For a period of three months he suffered from lymphuria, with intermissions. Then, little by little, the urine returned to its normal characters; and until lately, viz., after seven years, he had not had any further attacks, though he still looks very anæmic, and his blood appears very fluid and discoloured. In the course of these seven years the author examined the blood from the finger or the ear from time to time, and never found filaria embryos in it, or in the lymphous urine. As the blood examined had in every instance been taken from this patient in the daytime, and sometimes after noon, it was doubtful whether the periodical absence of the embryos from the blood might not be thus accounted for. Some days ago, however, a nocturnal examination was made at 9 p.m., but still no filariæ were found. So there is no doubt that this subject does not contain embryonal filariæ in his blood. Yet the characters of the lymphuria in this case resemble so much the other cases of filarial lymphuria which he observed, that Dr. Sonsino entertains still the doubt that they have the same and unique origin. But in what manner may be explained the absence of the embryonal filariæ from the blood of this lymphuric subject? The fact may happen in two ways: either the adult filariæ had come out with the lymph urine before the examination of the patient, or the patient was or is the host of adult filariæ of one sex only, so that there is no generation of embryos. That this last condition may occur no one can deny; and if the origin of lymphuria, or of other disorders, is due, as is most probable, to the adult worm or worms, and not to the embryos, lymphuria or other ailments from filariæ may occur without having any embryo in the body. This is a mere hypothesis, but it is not at all an unlikely one.

With reference to the diseases supposed to be due to the presence of the worms, only lymphuria, lymphocele and elephantiasis were met with, Dr. Sonsino not having yet met with an instance either of lymph-scrotum or the *Helminthoma elastica* of Bancroft. He thinks the commonly admitted

hypothesis, that these maladies arise from the mechanical obstruction and irritation produced by the adult worm in the lymph channels is capable of explaining the facts, but does not admit that it is the sole cause of the affections in question.

Though many persons infested with filariæ enjoy good health, the worm may possibly give rise to either thrombosis, embolism, or abscesses in important viscera, and hence should be considered a dangerous parasite, which may at any moment threaten the life of its host.

Differences between Filarious Hæmato-Lymphuria, and Bilharzia Hæmaturia, and other Disorders of the Urine.—The clinical features of filarious lymphuria are so different from bilharzia hæmaturia that generally they can be distinguished without hesitation, before having recourse to the microscope. Opaque urine, looking quite like milk, which curdles very soon after emission, if not before, is characteristic exclusively of lymphuria, which is due to lymphorrhagia in the tract of the urinary ways. As far as Dr. Sonsino knows, after an examination of many hundreds of urines of bilharzial subjects,—bilharzia never gives place to lymphorrhagia. A milk-like appearance of the urine may be assumed also when the urine contains much pus, or a very large quantity of phosphates or of urates, but in none of these cases does it curdle spontaneously. Besides, in the case of phosphates the opacity disappears on adding a little acetic acid; whilst in the case of urates the urine becomes transparent by heat alone. In filarious lymphuria sometimes there is blood, which appears like streaks in the mass of the liquid, or gives a rose or pinkish hue to the total of the liquid. In bilharzia hæmatobia disease, urine generally presents itself with its normal appearance,* a transparent, clear, light amber colour. In this case, only after standing are some yellow or dusky grey flocculi at the bottom perceived, in which the microscopical examination discovers the eggs of the worm. When there is hæmaturia, the blood generally tinges only the last drops of the urine, and it is exceptional that the urine is entirely bloody. The disorders of the urine due to bilharzia last for years, and when the disorder is of long standing the urine assumes generally the characters proper to the urine of cystitis, and sometimes also contains gravel, or is associated with symptoms due to stone in some part of the urinary tract. Filarious lymphuria, or

* *Ricerche intorno alla Bilharzia Hæmatobia in Relazione colla Ematuria Endemica dell' Egitto, e nota intorno ad un Nematoideo trovato nel Sangue umano.* Laono del Dott. P. Sonsino. Nel rendiconto della R. Accademia delle Scienze di Napoli. Fascicolo sesto, 1874.

hæmato-lymphuria, presents instead intermittent attacks, which have between them long periods of apparent recovery. Stone in the urinary tract is not a rare consequence or sequela to bilharzia disease, inasmuch as the author has ascertained that the flocculi with eggs of bilharzia may constitute themselves as nucleus of stone. He has no facts for suspecting that filaria disease may give origin in any manner to stone.

Co-existence of Filaria and Bilharzia, and Relative Frequency of these and other Worms in Egypt.—Filaria sanguinis and bilharzia hæmatobia are, without doubt, worms quite distinct. They may be found in the same subject, as the author has ascertained in two of his ten cases, but their association is quite accidental. Filaria sanguinis seems to be geographically more largely distributed than bilharzia. This latter has been found only in the African continent and in some of the near islands (Mauritius); it is doubtful if it is also found on the Arabic shore of the Red Sea; whilst, besides in Egypt, filaria is already known in many parts of Asia and America, and in Australia. But, as far as Dr. Sonsino knows, bilharzia is more common than filaria sanguinis in Egypt. Among not less than 300 boys examined in the Governmental school of Tantah in the year 1880, more than a third had hæmaturia, or had suffered from it. Of seventy-five autopsies which he performed on indigenous subjects in a period of six years, from 1875 to 1880, in Cairo, Zagazig, Benha, Mansoorah, and Tantah, and in many of which he made only a *coarse* search for the worm, he discovered its presence in thirty-eight subjects; but this result does not offer the just proportion of the infested subjects, as, being pressed by time, he was obliged to limit his examination often to the bladder only, and could not extend it to the other organs (ureters, spermatic vesicles, large intestines), which may present, too, the conditions due to bilharzia. But his conviction, from all the facts that he collected in his personal practice of nine years in this country, is that few persons in Egypt who drink habitually filthy water do not suffer from bilharzia in the course of a long life.

As for the frequency of filaria sanguinis parasitism in Egypt, there is no positive evidence to establish it, but, as already said, it is less than that of bilharzia. The author has examined the bloody urines (not lymphous) of many hundreds affected by bilharzia disease, and only twice has it happened to him to meet with specimens of filaria. If cases of this worm were more frequent, he would have met with its embryos more often in bloody urine. He has especially examined the blood taken by pricking a finger in many individuals affected with bilharzia, but till now in two cases only has he verified the existence of filaria embryos.

Dr. Sonsino was able to confirm the observations of Manson, Wykeham Myers, and Mackenzie as to the greater abundance of filariæ in blood taken at night, as compared with that taken during the day; and also as to the presence of filariæ in the stomach and tissues of the mosquito. He observed very slight differences, consisting in the formation of a nipple-shaped prominence, or sucker, at the oral end, and none of the metamorphoses described by Manson. Hence, he thinks the *rôle* of the mosquito is more like that of certain birds in the distribution of the seeds of plants.

As regards prophylaxis and treatment, as in the case of bilharzia, so with filaria, the drinking only of boiled or well-filtered pure water, is the best means of avoiding the disease. He knows of no radical remedy which will destroy the parent worm, and, hence, the treatment must be symptomatic, unless, as in elephantiasis, surgical interference gives hope of the removal of the cause.

For the treatment of lymphuria, he places most reliance on rest, with astringents, of which he prefers *tinctura ferri perchloridi*, and gallic acid internally; while the anæmia and wasting are treated by tonics and abundant nourishment. Urethral or vesical injections he considers of little use. Another worm which plays, with bilharzia and filaria, an important *rôle* in the production of disease in Egypt is the *Ankylostoma duodenale*, which, by the irritation it excites, as well as the hæmorrhage it produces, is the cause of the formidable malady known as "Egyptian chlorosis" (Griesinger), or "intertropical hypohæmia" in Brazil. Of other parasites, *Oxyuris vermicularis*, *Ascaris lumbricoides*, and *Tæniæ*, especially *T. inermis*, are very common, both in Europeans and natives. Hydatids are, also, not by any means so rare as has been supposed. *Dracunculus medinensis* is met with chiefly in Nubians and negroes, not in natives who have never been out of the Delta. *Tænia nana*, *Distoma heterophyes*, and *Trichocephalus dispar* were not met with by Dr. Sonsino, though he had one opportunity of observing the rare *Pentastomum constrictum* in a negro from Central Africa.

THE PROGRESS OF ZYMOTIC MICRO-PATHOLOGY.

By G. C. HENDERSON, M.D.

(*Read: Nov. 2nd*, 1881.)

THE recent discoveries of the existence of various minute organisms in different infectious diseases of man and of the lower animals, and the great interest excited by the question of their relation to the morbid processes with which they are associated, have induced me to lay before the Society this evening a short summary, in the main a historical one, of the present state of our knowledge.

Since the year 1850, in which Rayer and Davaine first described fine rod-like bodies, resembling vibrios in their optical and chemical properties, which they had found in the blood of animals dying of splenic fever, numerous observers have been working at the subject, and of late years, owing to improved methods of examination by means of staining the minute organisms with various aniline dyes, the list of diseases in which micro-organisms have been found, has been considerably extended. They have been found in the blood, lymphatics, or tissues, as well as in the special foci of disease, or in the discharges from them in :—

1. Anthrax, or splenic fever.
2. Septicæmia and pyæmia in various forms, *e.g.*, puerperal fever, ulcerative endocarditis, etc.
3. Erysipelas.
4. Diphtheria.
5. Scarlatina.
6. Variola.
7. Vaccinia.
8. Enteric fever.
9. Relapsing fever.
10. Malarial fevers, intermittent and remittent.
11. Leprosy.
12. Tuberculosis, in man and animals.
13. Pneumo-enteritis of pigs.
14. Fowl cholera.
15. "Spreading gangrene" of animals.

Micrococci have also been described as met with in the discharges of gonorrhœa and purulent ophthalmia, in the crusts of impetigo contagiosa, and in the primary hard sore of syphilis, and have been supposed to represent the special virus of the different diseases.

The micro-organisms occur in the form of :—

(1). Micrococci; small, round, or oval, highly refracting particles, either isolated, or aggregated together in masses, surrounded by a finely granular matrix, forming what is known as a *zoogloea*. Occasionally two are found united together, forming what has been termed a *diplo-coccus*. They multiply by fission, and are extremely minute, measuring not more than the one-twenty thousandth to one-thirty thousandth of an inch in diameter.

(2). Bacteria, short rods, of varying sizes, about the same diameter as micrococci, and about three or four times their diameter in length. Like the micrococci, they are frequently united in pairs.

(3). Bacilli, slender rods, usually of less diameter in proportion to their length than bacteria, but otherwise resembling them, and, like them, possessing the power of spontaneous movement, due to the oscillations of an extremely minute terminal flagellum or cilium.

(4). Vibriones and leptothrices, filaments apparently made up of a number of segments, like a number of bacteria or bacilli, joined end to end.

(5). Streptococci, another filamentous form, made up not of rods, but of minute beads, like a necklace of micrococci.

Both bacteria and bacilli, as well as micrococci, consist of a delicate external layer, composed apparently of cellulose, inclosing an inner mass of protoplasm. They increase in number not only by growing out into longer filaments, at first quite homogeneous, which, later on, become segmented, and break up into a number of separate small rods, but also by the formation of minute, highly-refracting, roundish or oval bodies, or spores, in the central portions of the filaments, which then undergo solution, and set free the spores. These latter, which appear to be much less easily destroyed than the rods, grow out, under favourable conditions, into rods, and thus complete the life history of the organisms.

Many organisms, which in appearance and mode of growth are identical or nearly so, give rise to very different products, or when introduced into the circulation or tissues of animals, to very different effects; and their reactions to different aniline dyes have also been made use of as a means of distinguishing them from one another, as well as of displaying

their existence more readily in the fluids and tissues in which they exist. This is notably the case with regard to the organisms of tuberculosis and leprosy.

First in order of discovery, as well as in the completeness with which its history has been studied, comes the disease known as anthrax, splenic fever, or malignant pustule, in England; and, in France, as *charbon*, or *sang de rate;* in Germany as *milzbrand.*

Soon after Pollender's first publication in 1855, the fine non-mobile rod-like bodies discovered by him were found in cases of splenic fever by many other observers. Opinions differed, however, as to the true nature of these rods, many considering them merely fibrin filaments, crystals, or impurities accidentally introduced into the preparations; and though Pollender found them in all the cases he examined, he expressed himself with great reserve in reference to their organised nature, and their relation to the disease. He observed, however, that blood containing them, when inoculated, was capable of producing splenic fever.

In 1857, Brauell found similar bodies in the blood and diseased tissues of men, sheep, and horses dying of anthrax, and published his opinion that their presence was diagnostic of the complaint. His observations and experiments were repeated and fully confirmed in 1863 by Davaine,* who found these bodies, which he named *bacteridia*, in every fatal case of *charbon.* He also noticed that on their disappearance, as the result of putrefaction, or by other means, the blood and fluids from the diseased tissues lost their infective properties. The anthrax fluids, even when diluted with a million times their volume of water, produced fatal splenic fever when injected into animals, thus behaving in quite a different manner to ordinary chemical poisons.

In 1866, Brauell, and soon after him, Davaine also, found that when pregnant animals died of anthrax, the maternal blood only contained numerous bacilli and was infective, while that of the fœtus was free from organisms, and could not transmit the disease. Here, the fact that the placenta appeared to form a true and efficient filter, strengthened the probability that the virus of the disease was associated with the organisms, and not with some purely chemical poison.

Bollinger, in 1872, discovered numerous bacilli on the feet and probosces, as well as in the intestinal canal of house flies, gad flies, and other insects in districts where anthrax pre-

* Davaine, with Rayer, had described the rods in 1850, but expressed at that time no opinion as to their nature.

vailed, and expressed his opinion that these creatures played an important part in conveying the infection.

In the following year, Klebs and Tiegel, after filtering the anthrax blood through porous porcelain, found that the filtrate, which contained no bacilli, produced no bad effects when inoculated, while the residue on the filter was capable of producing splenic fever.

In 1874, Dr. Burdon Sanderson, and Professor Cohn of Breslau, independently of one another, came to the conclusion that there must be two forms of organisms in anthrax, one, the rods, which were easily destroyed, and non-persistent, while the other, analogous to the "resting spores" of some other cryptogams, resisted unfavourable external conditions, and retained their power of growth after drying, and even exposure to great heat and cold, for a long time. In this way they explained the extraordinary tenacity with which the disease clung to certain localities and buildings, as well as one of Brauell's experiments, in which blood from an animal dying of anthrax, though no rods were found in it, communicated the disease to another.

This hypothesis received full experimental confirmation from Koch in the course of the years 1875-76. While cultivating the bacilli outside the body in suitable media, he found that the rods retained their vitality usually only for a few days, and at the longest five weeks, while the minute oval or roundish bodies, which he considered the *spores* of the organism, were capable of developing, even after four years' keeping, into filaments and new spores when cultivated in an incubator. The fluids containing these bodies could produce anthrax in mice, while if no bacilli were developed, its inoculation was harmless. While the question stood at this stage, Pasteur, who for several years had been occupied with the study of fermentation, repeated and confirmed the filtration experiments of Klebs and Tiegel. In 1877, he cultivated the bacilli in neutralised sterile urine, and stated that the spores resisted even boiling, and treatment with condensed oxygen at a pressure of ten to twelve atmospheres.

In 1878, Dr. Cossar Ewart, on repeating Pasteur's experiments, found that these last conclusions were erroneous. He also described and figured the process of development of the spores into distinct bacilli, and noticed the interesting facts that the addition of water containing the ordinary Bacterium terma (of putrefaction), as well as decomposition of the nutrient solution, destroyed the fully-developed bacillus anthracis, but not its spores, while drying at a temperature of 98 deg. to 104 deg. F. (37 deg. to 40 deg. C.) did not affect either spores or rods to any great extent.

In 1879, Pasteur, continuing his previous researches, stated that *birds*, which he had found insusceptible to anthrax, succumbed to the disease when their temperature had been artificially reduced in a refrigerator. In his opinion the temperature of their blood, which was much higher than that of mammalia, proved fatal to the bacilli, and hindered the development of the spores. Neither his observations, nor the explanation he offered, have subsequently been confirmed; in fact, in 1879-80, Oemler and Huber found that sparrows could be pretty readily infected with anthrax. They also noticed that 8 out of 28 ducks, 15 out of 38 pigeons, and 11 out of 31 fowls—the very species of which Pasteur had asserted the immunity, succumbed to anthrax when inoculated. The explanation is also negatived by the fact that Pasteur himself, in the course of the year 1881, described a pretty free growth of bacillus anthracis in media kept at a temperature of 107 deg. to 108 deg. F. (42 deg. to 43 deg. C.) which is about the average temperature of the blood in birds. Moreover, as Koch pointed out during the latter part of 1881, he does not seem to have shown whether the fowls he used were really proof against anthrax, by inoculation before their temperatures had been reduced.

In the same year (1879), he ascribed the origin of cases of spontaneous anthrax to the inoculation of abraded surfaces in the mouth and œsophagus of the animals affected, as he found that the cervical lymphatic glands in these cases, as well as in animals fed with infected fodder containing thistles and furze, etc., became inflamed and swollen. This mode of infection had, however, been noticed as early as 1850, by Heusinger in his work on splenic fever.

In a communication to the Académie de Médecine in November 1879, Pasteur insisted that the transport of spores by earthworms from the buried carcases of animals dying of anthrax to the surface of the ground was the main reason of the persistence of the virus in certain country districts. Koch, however, in an extremely searching criticism published in October 1881, assigns several reasons for doubting the validity of Pasteur's conclusions. The experiment which Pasteur made was as follows :—After making an autopsy of a sheep which had died of spontaneous anthrax in a garden of the farm belonging to M. Manoury, the body was buried in the same place. Earth taken from the surface of the soil ten and fourteen months later caused anthrax when inoculated into guinea-pigs. Pasteur assumed that the only way in which germs could have reached the surface was by transport by earthworms from the buried remains. He overlooked,

however, a fact which he himself, in common with other observers, had noticed; viz.—that before death there are often discharges of bloody fluid from the mouth, nose, and anus, and frequently that the urine also is sanguinolent, so that in almost every case, the earth round the corpse has been infected with blood containing numbers of bacilli and spores. The autopsy also gave further opportunity for infecting the surface soil, beneath which the animal was finally buried, so that there is no need to assume the transport of germs to it by earthworms in order to explain the subsequent production of the disease. For the experiment to warrant the conclusion, the body should have been buried in a spot proved by inoculation experiments to be free from anthrax germs, and the greatest possible precautions should have been taken to avoid contaminating the surface with blood, washing, etc. For a further consideration of Koch's criticisms, as well as to the experiments from which he concludes that earthworms play a very subordinate part in disseminating the virus, I must refer you to the original in the *Mittheilungen aus der Kaiserlichen Gesundheitsamt*, Berlin, 1881.

In 1879, Dr. Sanderson and Mr. Duguid, after transmitting anthrax from bovine animals to guinea-pigs, and re-inoculating the former from the rodents, came to the conclusion that this retro-inoculation caused only a modified form of anthrax, not fatal to cattle, but capable of protecting them when inoculated with the unmodified virus. Soon after the publication of their results,* Dr. Greenfield, in his Brown Lectures in 1880, confirmed Dr. Sanderson's observations, and found further, that in cultivating anthrax bacilli outside the body, the earlier generations (I to III) retained great virulence, while the later ones (VIII to XIII), caused only slight symptoms. Animals which had survived the inoculation of the third generation, were proof against subsequent inoculations, and did not suffer when sent to pasture in fields infected with anthrax. In the same year, Pasteur, following up the method he had employed in *fowl cholera*, stated that he had succeeded in reducing the potency of anthrax virus in the following way. He found that the bacillus would not develop in fowl broth when kept at a temperature of 45 deg. C. (113 deg. F.), but that at a temperature of 42 deg. to 43 deg. C. (108 deg. to 109 deg. F.), it grew pretty vigorously, *but developed no spores.* Its virulence progressively diminished till, in about a month, it became exhausted, and died out. If animals were inoculated two or three days before the cessation of virulence, they survived and were *vaccinated* against subsequent attacks of the disease.

* *Journal of the Royal Agricultural Society*, vol. i, 1881.

This modified virus, though it was innocuous to adult guinea-pigs, killed those only a day old; and on retro-inoculation to other animals gradually increasing in age, at each transmission it increased in potency till it recovered its original virulence. Toussaint at the same time, after trying, and abandoning as untrustworthy, the filtration of anthrax blood, stated that after heating it to 55 deg. C. (131 deg. F.), and adding 1 per cent. of carbolic acid, inoculation produced in animals only a modified form of anthrax, which, like Pasteur, he found to be protective. Almost simultaneously, Chauveau, by repeated inoculation of Algerian sheep (which possess a certain insusceptibility) with unmodified virus, stated that he could make them completely proof against anthrax; and that the lambs borne by ewes inoculated during pregnancy were also unaffected by inoculation after birth.

In March 1881, Colin, while repeating Toussaint's experiments found that two out of five rabbits succumbed to the preventive inoculation, while the remaining three died later on, when inoculated with the unmodified virus. Some weeks later, Buchner published in the *Deutscher Medicinische Wochenschrift* some experiments on the cultivation of *bacillus subtilis*, of hay infusions, in solutions containing peptones or Liebig's extract of meat, from which he concluded that he had succeeded in obtaining bacilli agreeing in every respect with those of splenic fever. Should his observations, which have hitherto remained without confirmation, prove correct, the hypothesis thrown out in 1877 by Dr. W. Roberts of Manchester that *bacillus anthracis*, and possibly other pathogenic bacteria, might prove to be "spores" of some ordinarily innocuous variety, will have received striking verification. It is by no means improbable, however, that his results, like those of Grawitz on the accommodative modifications of *Penicillium*, *Eurotium* and other fungi, are capable of another explanation. During the course of 1881-82, under the direction of Dr. Roszaheghy, the Austro-Hungarian government have ordered an extensive trial of the methods of Pasteur and others for protective inoculation. The results up to the present time seem to show that the preventive inoculation itself, in many cases proves fatal; and that when the animals survive, the immunity it confers on them is only for a short period, the duration of which is very variable.

2. *Septicæmia and Pyæmia.*—Under the heading of septicæmia two very different processes have commonly been included, viz.:—

(1) The effects produced by the introduction into the body of the chemical products of putrefaction; (2) the results of the

entry of the minute organisms commonly met with in decomposing fluids. The former, for which the Committee of the Pathological Society on Pyæmia have adopted the term of *septic intoxication* or poisoning, resembles the ordinary chemical poisons in its mode of action; its effects are in proportion to the amount given, and it does not seem in any way to become intensified in the body. The latter, *septic infection*, is a contagious process, communicable from animal to animal by inoculation, and producing effects out of all proportion to the dose introduced into the system. It is this process alone with which we are concerned.

In 1866, Rindfleisch described micrococci as constantly present in great numbers in the infarcts and abscesses in pyæmia, "surgical kidney", septicæmia and puerperal fever; and in the same year Coze and Feltz confirmed his observations. They also stated that in artificial septicæmia, produced by the inoculation of putrefying meat infusions, the intensity of the virus increased at each transfer when the disease was inoculated from animal to animal.

In 1871, Von Recklinghausen and Waldeyer simultaneously described the presence of micrococci in pyæmia, "surgical kidney", and other septic affections. In the following year, Klebs, Birch-Hirschfeld, and others, found them in the discharges from unhealthy wounds, and on the granulations, joint surfaces, and serous membranes of persons suffering from septicæmia. Klebs traced them from the surface of a wound into the connective tissue spaces, and found them also in an adjacent thrombosed vein, in the clots behind the valves of veins, and in the metastatic infarcts.

Davaine, in 1872-73, repeated and confirmed the experiments of Coze and Feltz, finding that after the 24th transmission of septic virus through rabbits, a trillionth dilution was sufficient to cause fatal effects. It has hence been assumed that the augmentation of intensity was a gradual and regularly progressive one; but Davaine himself gives a series of observations, in which the full intensity was obtained in two or three transmissions, and the conclusion in his own words is, "Ces faits prouvent suffisamment que le virus septicémique acquiert tout de suite sa plus grande virulence."

Dr. Burdon Sanderson in the same year, 1872, found in the peritoneal fluid obtained from guinea-pigs dying of infective inflammation, numerous fine glistening rods and micrococci, singly and in chains. While confirming the augmentation of virulence of the septic fluid in transmission from animal to animal, he expressly states that it "does not

take place by successive steps, but *per saltum*". Like anthrax blood, the liquid after filtration through porous porcelain presented no organisms, and was non-infective; putrefaction also destroyed its special properties.

Heiberg, in 1873, described micrococci in the valvular lesions, and emboli of ulcerative endocarditis, and in the metastatic foci of puerperal fever; and the different publications of Billroth and Hueter between the years 1872-74, gave further proof of the frequent occurrence of organisms in septic diseases.

In 1878, Dr. R. Koch, by the use of methyl-violet and other anilin dyes, together with special methods of illumination, succeeded in demonstrating readily the presence of micrococci, bacilli, and filamentous chains in the tissues and blood of animals dying of different forms of septicæmia and spreading gangrene. He found that each definite clinical variety of the disease was attended and associated with the presence of a special form of micro-organism, which could be cultivated outside the body in suitable media for several generations, and on re-inoculation reproduced the original disease without any diminution of virulence. By growing the fungi on a gelatinous medium instead of in liquids, he was able to get crops of them free from the admixture of other fungi derived from particles in the air, which in fluids became intimately mixed up with the specific organisms, and either lessened or destroyed their special properties.

In the continuation of his researches, with the co-operation of Drs. Gaffky and Loeffler, published in September 1881, he endeavours to show that in so-called "pure cultivation", in semi-solid media, the specific micro-organisms "breed true", reproducing bacilli, etc., of the same form, and with the same properties as the original stock; that there is no diminution in the intensity of the virus; and that the modification of the virus in anthrax and fowl cholera in Pasteur's cultivation experiments, was due to the introduction of foreign particles with germs of other organisms, which, on developing, gradually destroyed the specific virus, overpowering it in the same way as the common bacterium termo does.

Similarly he explains the intensification of virulence noted in Davaine's and Sanderson's experiments. The original virus—decomposing blood, muscle infusion, or the products of the injection of liquor ammoniæ—contains not one, but several species of organism, and hence on subsequent inoculation, there is a struggle for existence between them, which ends, at the second or third transmission, in the mastery of the most active species, and, in its subsequent unmixed inocula-

tion. His experiments tend to show that under the common name of *septicæmia*, as formerly of "continued fever", we have several different varieties of disease, which a more careful clinical and pathological study may subsequently enable us to distinguish. Almost at the same time as the last writers, Semmer and Krajewski published a short note, in which they asserted that they succeeded by the methods of Pasteur and Toussaint, in conferring upon animals a certain degree of immunity from septicæmia.

3. *Erysipelas.*—The first mention of the occurrence of micrococci in erysipelas, was made by Nepveu in 1872. He found them in the blood of his patients, and specially numerous in that taken from the inflamed part. During the course of the next four years, they were described by Wilde and Orth as present in the pus of unhealthy wounds from which erysipelas had started, and in the fluid from the cutaneous bullæ as well as in the blood. Somewhat later, Recklinghausen and Lukomsky found them in the lymph canaliculi and lymphatics of the skin, most numerous near the spreading margin of inflammation, but also radiating away beyond it for nearly two inches. Billroth and Ehrlich also described their occurrence in the blood-vessels, and soon after, Tillmanns and Letzerich found micrococci in the muscles, liver, spleen, and kidneys of patients dying of erysipelas. Orth also succeeded in transmitting the disease to rabbits by the inoculation of fluid from the bullæ on the human skin; and in 1878, Koch found that the complaint was readily communicable by inoculation from animal to animal.

4. *Diphtheria.*—Since micrococci were described by Letzerich as present in the false membranes of diphtheria, numerous other observers, including Oertel, Billroth, Klebs, Eberth, Nassiloff, and Trendelenburg, have succeeded in finding them in the deeper layers of the affected mucous membrane, in the lymphatic vessels and glands, and in distant organs, especially the kidneys.

In 1874, Oertel, after introducing small fragments of diphtheritic membrane into the tracheæ of rabbits, found that the latter died in about thirty-six hours, after presenting symptoms of croup. Whether the material was introduced into the trachea or beneath the skin, false membranes were developed in the air passages. The affection thus produced was communicable to other rabbits and to pigeons by inoculation.

Oertel's statements remained without confirmation till 1881, when Talamon found that the micrococci could be

cultivated, like those of anthrax, outside the body, and reproduced the disease on inoculation.

5. *Scarlatina.*—In 1875, Dr. Klein described and figured masses of granular bodies resembling micrococci in cavities of the epidermis, and in sections of skin taken from patients who had died while the rash was most intense. In the throat and tonsils, he confirmed Dr. McKendrick's statements made in the same year, that micrococci were abundant, both on the surface and in the interior of the organs. Inoculation experiments on animals, when portions of tissue and fluid from the diseased parts were used, gave rise to septicæmia only, and not scarlatina.

6. *Variola.*—Micrococci were observed by Weigert in 1871 in the lymph obtained from small-pox vesicles, in the walls of the vesicles, and in the adjacent lymphatics. Dr. Klein, in 1874, also found them in ovine variola, and in 1876 maintained his views as to their existence, though he withdrew his opinion as to the occurrence of higher forms of fungi in sheep-pox.

7. *Vaccinia.*—In vaccinia, Dr. Keber of Dantzig, described micrococci in vaccine lymph, and stated his opinion that they were the carriers of the virus, if they did not themselves form the contagion. Similar bodies were also described and figured in 1870 by Drs. Sanderson and Klebs; and two years later Cohn gave an exhaustive account of them.

Mr. R. J. Godlee, in 1878, succeeded in cultivating the organisms of vaccine lymph outside the body; but found that after several generations they had lost their infective power, though they had not changed in form, and were apparently free from contamination with other fungi.

8. *Enteric Fever.*—In this disease, Dr. Klein, in 1874, found micrococci in the fresh stools, on the surface and in the substance of the intestinal mucous membrane, in the lymphatic spaces and vessels, and in the small blood-vessels of the intestine, as well as in the substance of the mesenteric glands, and in the capillaries supplying them. Monkeys and various other animals did not develop the disease after the ingestion of milk, which had purposely been infected.

In 1880, Klebs stated that he had found a short, thick bacillus in the intestines, lymphatic glands, lungs, larynx, kidneys, and pia mater, of patients who had died from enteric fever. It could be cultivated outside the body, and when subsequently injected into rabbits, caused a bacillus infiltration of their tissues exactly similar to that seen in man. The micrococci which he also found, he thinks, are due to septic processes, starting from the intestinal ulcers. With

regard to his inoculation experiment, it is not stated whether the symptoms during life, and the naked eye *post-mortem* appearances resembled those met with in the disease of man.

In 1882, Dr. Coats of Glasgow, confirmed Klebs' statement of the existence of the bacillus, which has since been found by other observers.

9. *Relapsing Fever.*—Since Obermeier of Berlin discovered and described in 1872 the organism which has been called, after him, Spirillum Obermeieri, in the blood of patients suffering from relapsing fever, it has been noticed by several other observers in epidemics of that disease in Germany, Russia, and India. The spirilla present in the blood during the primary paroxysm, disappear at the crisis of the fever, are absent during the intermission, and reappear during the relapse. On inoculation, the blood which contains them gives rise to the disease both in man and in monkeys, in which animals, Dr. Vandyke Carter in 1877 and 1878, succeeded in transmitting the affection through a series of individuals. Heydenreich in 1877, made some interesting observations on the behaviour of spirilla outside the body. At the temperature of the air, they retained their movements for several days; in an incubator at the normal temperature of the body (37 deg. C., or 98.6 deg. F.), they died in from twelve to fourteen hours; and at a temperature of 40 deg. C., or 104 deg. F., they had all succumbed in four to twelve hours. He therefore concludes that the spirilla seen during the paroxysm are the result of several different crops of spores, a circumstance which may account for the sudden variations in their numbers noticed at different periods of the attack.

Dr. Vandyke Carter in 1878-79, in the course of his experiments on monkeys, found that one attack of the disease conferred no protection whatever against subsequent inoculation; and in the same year (1879), Koch also stated that neither immediately after convalescence, nor some weeks later, was there any indication of immunity having been conferred by one attack of the disease.

10. *Malarial Fevers.*—In 1879, Klebs and Tommasi-Crudeli, working in the Roman Campagna, found a bacillus in the spleen and blood of persons suffering from malarial fever, in the air of malarious districts, and in the water and washings of the soil from such districts. Injection of fluid containing these bodies into the veins of animals, or their ingestion by the mouth, caused fever, with rigors, swelling of the spleen, and other symptoms resembling those of ague; and

on examination of the blood and tissues, they were also found.

In the following year, Lanzi and Terrigi, as well as Marchiafava and Cuboni confirmed these observations. An organism, differing, however, in several respects from that described by the previous observers, was also found by Laveran in cases of malarial fever and cachexia in Algiers. Dr. Sternberg, in examining the water and soil from marshy places in the Southern States of America, where ague was prevalent, found several species of organisms, some of which agreed in appearance with those described by Klebs, but he did not succeed in producing intermittent fever by inoculating them into animals.

11. *Leprosy*—Towards the end of 1879, Dr. Neisser of Breslau, who had been examining cases of leprosy in Norway, published descriptions and figures of bacilli in sections of leprous tubercles; and in January 1880, Dr. Hansen of Bergen gave a continuation of his researches on leprosy, published in 1874, and stated that he had observed numerous bacilli in the cells of leprous nodules, while the dark brown pigmented cells noticed in the skin by many observers, and in the nerve tubercles by Dr. Vandyke Carter, he found, on staining with anilin violet, to be crammed with bacilli; no micrococci were seen in the fresh blood, though in moist preparations kept for a few days in the incubator there appeared fine articulated filaments, which did not appear in blood, specimens of which were taken from healthy and syphilitic individuals, and treated in the same way.

In 1881, Cornil found the nodules and other affected portions of the tissues of lepers contained numerous very fine bacilli.

12. *Tuberculosis.*—Though the inoculation experiments of Villemin and Wilson Fox in 1866 and 1867, followed by those of Sanderson, Klein, Cohnheim, and many others, had gradually accumulated strong evidence of the communication of "tubercle" to animals by inoculation, and though Klebs and Schüller had as early as 1873-74 described micrococci as present in the miliary granulations, it was not till the end of 1881 that Koch, by a special mode of staining, succeeded in positively establishing their existence. He found a very minute bacillus in the "grey granulations" of tubercle, and after cultivating it for several generations in suitable media, at a temperature of about 100 deg. F., it gave rise to tuberculosis when inoculated into animals. Ehrlich, Heneage Gibbes, and others have since shown the same organism in the sputum of phthisical patients, and it has also been de-

tected in the condensed moisture of their breath. Aufrecht and Baumgarten have also described similar bacilli in the "perlsucht", or bovine tuberculosis by different methods of examination. As yet, Koch's experiments on the artificial cultivation of the bacillus, and of the subsequent production of tubercle by it have remained without control from other observers.

In gonorrhœa, a micrococcus was found in the discharges by Neisser in 1879, and Bôkai in 1880, as well as by Watson Cheyne in 1881; and a bacillus has also been described by Aufrecht in syphilis, and by a Brazilian observer, in 1882, in cases of yellow fever. Ogston in 1881 also found micrococci in the pus and in the granulations lining the walls of acute abscesses, and after cultivating them for several generations in dilute albumen solutions, observed that their inoculation produced similar abscesses in animals.

Among the infectious diseases of animals the contagious pneumo-enteritis of pigs and the so-called "fowl cholera" have been the most thoroughly investigated. In the first-named malady, the communicability by inoculation was definitely settled by Professor W. Axe in 1875, as well as the points of difference between it and anthrax, with which it had long been confounded.

In the following year, in a preliminary report, and more completely in 1878, Dr. Klein gave an account of his experiments on the communicability of the disease by inoculation with the tracheal and bronchial mucus, diseased portions of lung, intestine, and the peritoneal exudation, while in only a few cases did the blood appear to be infectious. The inoculated disease, like variola in man, and pleuro-pneumonia in cattle, communicated in the same way, was of a mild type. In the fresh blood, in nearly all cases, he found no organisms, though in one case, the only one which produced the disease on inoculation, both rods and spores were present in the blood. The spleen was also free from organisms, but in the lungs, on the bronchial mucous membrane, as well as in the alveoli, he found them, and on the surface as well as in the fluid effusions of the pleura and pericardium. These organisms grew well outside the body, and retained their power of reproducing the disease even at the thirteenth generation. From a careful consideration of the lesions, Dr. Klein concludes that the affection is specifically distinct, both from human enteric fever and from anthrax.

Dr. W. Osler, of Montreal, repeated and confirmed Dr. Klein's results in 1878.

"Fowl cholera," which Perroncito some years ago had described carefully, was shown in 1879 by Toussaint to be connected with the development of a special form of organism in the diseased tissues. In February 1880, Pasteur described the cultivation of this organism in chicken broth which had been previously sterilised and made alkaline. He stated that if the intervals between each generation were made pretty considerable, fifteen days to one or two months, the activity of the virus lessened, presumably under the influence of the oxygen of the air, till at last a cultivation was obtained which no longer proved fatal to fowls, but rendered them insusceptible, for a pretty considerable period, more than a year in some cases, to the ordinary contagium, or to inoculation with the unmodified virus. In later communications he has stated that not one, but several protective inoculations were necessary to complete the immunity of the fowls. Though, perhaps, it may fall somewhat outside the exact limits of the subject we are considering this evening, I cannot refrain from giving a brief notice, from Dr. Loeffler's article, of the artificial cultivation of ordinary filamentous fungi, and their behaviour when subsequently introduced into the circulation.

It was in 1870 that Grohe and Block noticed that after injecting spores of species of Eurotium and Penicillium into the veins of animals, in a few days a general mycelial growth took place in different organs, and death ensued. No confirmation of this statement appeared, and Grawitz, in 1879, after various modifications, came to the conclusion that, even after injection of considerable quantities of spores of the above-named fungi, no mycelial development occurred. But, after gradually accustoming the fungi to grow on media containing blood instead of bread, potato, etc., he found, after twelve to twenty generations, that spores were produced which behaved exactly as Grohe had stated. In this way he believed he had produced a physiological variety of the common innocuous fungus, which was able to overcome the resistance of animal tissues. It appears, however, that there are some species of fungi which, even without physiological modification, are able to get the upper hand when introduced into the animal tissues, chiefly those belonging to the genus Aspergillus (glaucus, nigricans, flavescens, and fumigatus), and numerous cases of mycosis produced by them are on record. In repeating the experiments, Loeffler could get results with Aspergillus nigrescens neither before nor after cultivation according to Grawitz's method.

During the present year, Dr. Krannhals of St. Petersburg,

stated that he had obtained the same results as Grawitz by cultivating Penicillium glaucum. Loeffler, after many failures, succeeded at last in getting spores to form in the media in which he was cultivating Penicillium, but, on microscopic examination, they were found to consist of Aspergillus glaucus, and not Penicillium. Some of these, when introduced into the jugular vein of a rabbit, produced most typical general mycosis. On cultivating scraps of the affected organs, Aspergillus was always developed. This test, which was not applied by Grohe or Grawitz, shows distinctly whether the mycelial invasion is due to the species of fungus, the spores of which were cultivated and injected, or to some other variety. On referring to the cases recorded, they are nearly all described as due to species of Aspergillus, and in this genus we have a plant which, unmodified, can accommodate itself to the conditions of the animal body. Hence, it is possible, as Loeffler suggests, that Grawitz's results may have been due to contamination with Aspergillus spores, and the possible fallacy, through not testing his mycelium, shows us how careful we should be, by control experiments, to exclude as many sources of error as possible.

What conclusions are we to draw from the presence of micro-organisms in the diseases already mentioned? Are they the causes of the various processes; are they merely associated or epi-phenomena; or are they merely accidental complications of the processes? In helping our decision, we have three sets of facts which tell in favour of a causal relation. 1. The organisms in either the blood, some of the tissues or organs, or in morbid fluids in animals suffering from the disease, whether developed spontaneously or after inoculation. 2. Blood, or fluids containing them or their spores, transmits the disease when inoculated; when they are absent, or have been artificially separated, inoculation gives no such result. 3. They can be cultivated, still preserving their specific form, outside the animal body, and after five, ten, fifteen, or more generations reproduce the original disease when inoculated. All these three kinds of evidence are found in the case of anthrax, some varieties of artificial septicæmia, pneumo-enteritis of pigs, and fowl cholera; and we have as strong reasons for believing that the organisms are the causes of the diseases in question as for holding that the ingestion of cysticerci in measly pork or beef is the cause of the tapeworms we subsequently find in the intestines.

With regard to relapsing fever certainly, and probably variola, vaccinia, erysipelas, diphtheria, and possibly

enteric fever also, if Klebs' observations are correct, we have only the first and second kinds of evidence; hence, though there is a strong probability that the organisms do cause the diseases, we cannot look upon the question as absolutely settled till experiments on artificial cultivation and retro-inoculation have been made. Yet, though trichinæ have not been "grown pure" in artificial media, no one doubts that their development and migration into the tissues are the cause of the general disturbance of the system known to us by the name of trichinosis.

In the case of scarlatina, ague, and leprosy, we have as yet no positive evidence of the nature of the organisms described, as afforded by inoculation and cultivation in the first and second class of diseases. While, therefore, from the analogy of other diseases, and from the gradual elucidation of one point after another in their etiology, we may hope for a future settlement of the question, we are not at present in a position to decide whether they are causes or consequences of the maladies in which they occur.

Another point, which remains for future discoverers to explain, is the reason why some zymotic affections, *e.g.*, variola, scarlatina, and measles, appear, after once attacking an individual, to confer immunity on him for a longer or shorter period of his subsequent life, while others, such as relapsing fever, influenza, and perhaps cholera, either confer no such immunity, or even, as in ague and erysipelas, seem actually to render him more liable to subsequent attacks. This question, as to the protectiveness or non-protectiveness of a primary attack of zymotic disease, is of crucial importance to the theory of protective or modified inoculation, which Pasteur has termed vaccination. If the unmodified virus itself does not confer immunity from future attacks, it is most improbable that a modified virus will do so; and although Pasteur and Chauveau, from their experiments on cattle and sheep, have concluded that anthrax is protective, the question does not yet seem to have been decisively settled.

There are numerous cases, quoted by Bollinger, Heusinger, and others, in which shepherds have had two, or even three, attacks of malignant pustule; and Oemler, in a series of papers published in the *German Veterinary Journal*, 1876-80, states expressly that in no instance could he find a true development of immunity by inoculations. Animals which had successfully withstood two, three, six, or more inoculations with unmodified anthrax material, in the end succumbed to the disease when the virus was given in larger doses. So

far as regards the immunity of cows after one attack of anthrax, Pasteur's opinion rests on *two* cases; of four animals inoculated two died, and the survivors resisted a second inoculation with unmodified virus. In Chauveau's experiments on Algerian sheep, he found that six out of eight of his animals, which had successfully resisted repeated small inoculations with unmodified virus, finally succumbed, after intravenous injection, to the disease.

In the face of Oemler's experiments, published in the *Archiv für Wissenschaftliche und Praktische Thierheilkunde*, which were made on cattle, dogs, cats, horses, and other animals, we cannot accept the conclusions of Pasteur and Chauveau as absolutely certain, but are obliged to conclude that though the animals in question (and their number is small), showed great resistance to repeated inoculations, their complete immunity has not yet been proved.

In a recent article, to which I have already more than once referred, Dr. Loeffler gives an account of experiments which tend to throw some doubt on the supposed immunity of the offspring of animals which had contracted anthrax before or during pregnancy. While, therefore, the foundation of the theory of protective inoculation is thus disputed, we shall only be justified in regarding the superstructure with some amount of suspicion, and in waiting till more extensive observations have either confirmed or disproved it. Unfortunately, the conditions under which scientific investigations are necessarily carried out in this country give us but few opportunities for attempting to settle this question.

The question as to whether the view of which Cohn is the most prominent supporter is correct, viz., that the organisms met with in the diseases we have been considering are specific, differing altogether from the ordinary bacteria which abound in the air, water, and soil, and are under ordinary circumstances harmless, or whether that of Nägeli, that the pathogenic organisms are modifications of more common innocuous ones, is at present far from settled, and both with a view to the prevention and the cure of the diseases, demands more careful investigation than it has yet received. If the nature of the medium, rather than the presence of organisms, is the more important factor, our endeavours should be directed to the object of making the former unsuitable for the development in certain directions of these bodies, as well as to checking their effects when the modification has taken place.

Disappointing as it may seem to have the brilliant prospect opened up to us by M. Pasteur's discoveries already obscured

by the clouds of doubt, it may be well to compare the present position of zymotic pathology with that of 1866, when Hallier's statements appeared to have opened up a new and promising field; and by appreciating the marked and solid gains to our knowledge since that time, to expect from the present conflict of opinions even more valuable results in the future.

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